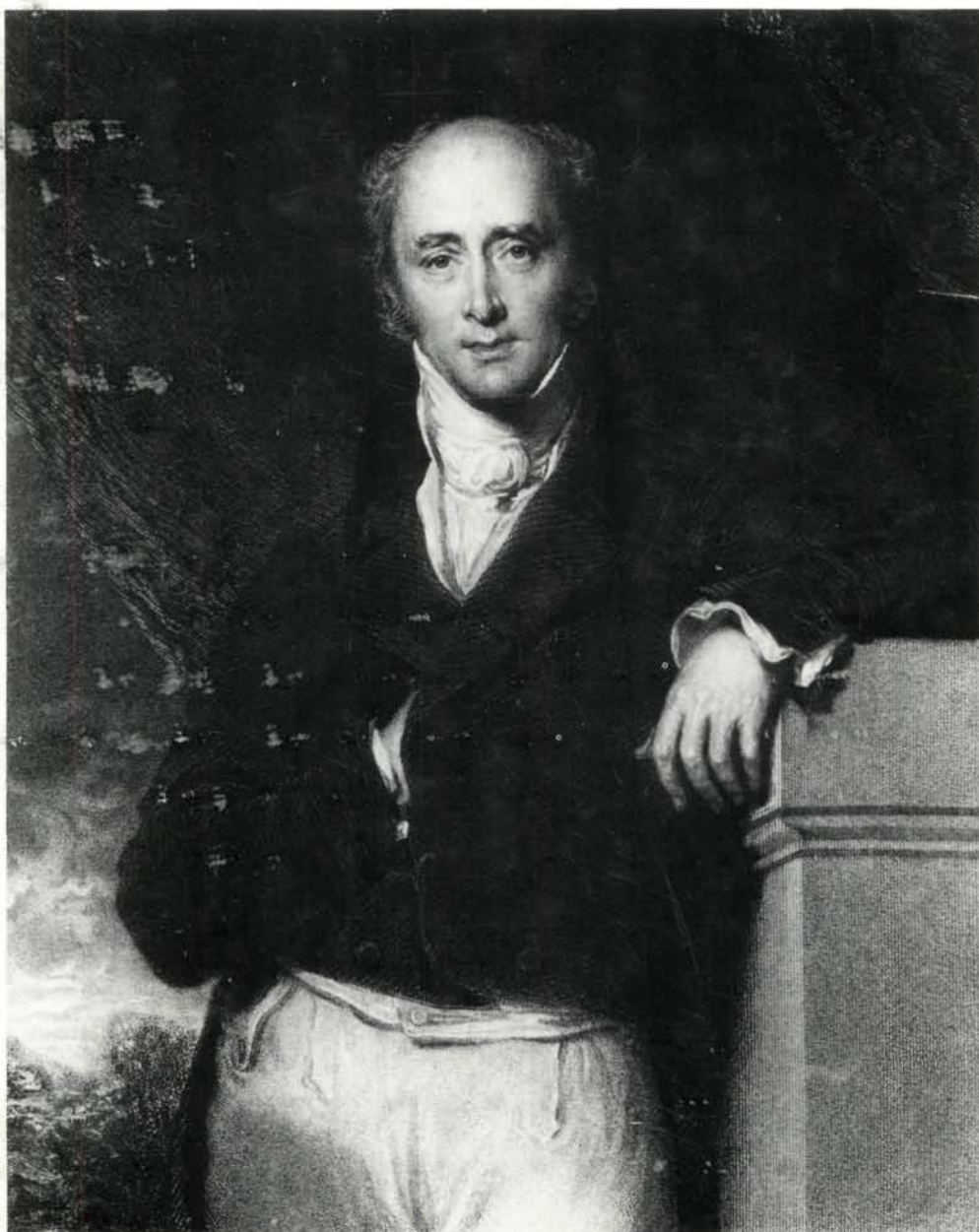


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# The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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## Ideology and Theory: The Tension between Political and Economic Liberalism in Seventeenth-Century England

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JOYCE APPLEBY

DURING THE LAST TWO DECADES of the seventeenth century—ninety years before the *Wealth of Nations* appeared—a number of British writers challenged the central premises of the balance-of-trade theory for economic growth. The currency crisis and the craze over Indian cottons had sharpened a sense of conflicting interests among Englishmen, and these divisive issues called forth a body of writing which attacked the principles underlying the mercantile system. Examining in a new way the operation of the market, Dudley North, Nicholas Barbon, Dalby Thomas, Henry Martyn, Francis Gardner, James Hodges, Henry Layton, John Houghton, and several anonymous pamphleteers produced explanations of economic relations which were far more sophisticated than the prevailing theories, anticipating at many points the premises of Adam Smith's monumental synthesis.

Yet despite this new plateau in economic reasoning, the conceptually flawed balance-of-trade theory, with its built-in corollary that economic regulation was essential to national security, became even more firmly fixed in the public mind in the eighteenth century. The ideological implications of the rejection of these writings have not been explored. Scholars have assumed the science of economics had to wait for the path-breaking geniuses<sup>1</sup> or that the balance-of-trade critics were too exceptional to treat as a significant group.<sup>2</sup> Charles Wilson recently described Dudley North as a swallow who did not produce a summer.<sup>3</sup> The analogy is worth pursuing. Taking a closer look at the birds in hand, it is possible to conclude that it was not the lack of swallows that counted, but the distaste for summers, that, in fact, economic thought ran ahead of social developments, and the analytical insights of the

I am indebted to William Kennedy, Margaret Gay Davies, Richard Steele, and, especially, Andrew Appleby for their careful reading of this article in manuscript.

<sup>1</sup> Eli Heckscher, *Mercantilism* (London, 1935), 1: 104ff; Jacob Viner, *Studies in the Theory of International Trade* (New York, 1937), 90, 117–18. This interpretation is implicit in Bruno Suviranta, *The Theory of the Balance of Trade in England* (Helsingfors, 1923). See also Alexander Gerschenkron, "History of Economic Doctrines and Economic History," *American Economic Review*, 59 (1969), 2; and William Letwin, *The Origins of Scientific Economics* (London, 1963), 144–48.

<sup>2</sup> J. D. Gould, *Economic Growth in History* (London, 1972), 220–22; Charles Wilson, *England's Apprenticeship, 1603–1763* (New York, 1965), 184.

<sup>3</sup> *Ibid.*, 266.

balance-of-trade critics were dismissed because they threatened the fragile social order in England during the country's critical passage into a fully capitalistic society.

The balance-of-trade explanation of how nations grow wealthy had focused attention upon production in such a way as to obscure the dynamics of consumption. Inside England the most noticeable consumers were the very rich and the very poor, and there was little in their patterns of spending to encourage a re-evaluation of consumption. As landowners, the rich could tap agricultural revenues first. Their rent rolls were the principal source of capital, but they spent rather than invested their income.<sup>4</sup> The very poor were a conspicuous drain upon the economy because there were so many of them, and their subsistence needs were paid for through taxes.<sup>5</sup> Gregory King's estimate that half the families in England could not pay for their living indicates the dimension of the problem of underemployment.<sup>6</sup> These realities made plausible the argument that since domestic consumption took from the store of English capital through luxury buying and the maintenance of the poor, markets for English goods should be sought outside the country. In other words, let the social overhead and upper-class vanity of other nations return a profit to England. Such a prescription fit well with the endemic political rivalries of seventeenth-century European states. Blocked from appreciating the role of domestic consumption, economic thinkers slipped easily into the assumption that consumption was a necessary evil, growing—if at all—in response to population growth.

Thomas Mun, Gerald de Malynes, and Edward Misselden had analyzed the influence of demand upon prices in their famous debates over the foreign exchange in the 1620s. In *England's Treasure by Forraign Trade*, Mun also drew attention to the way elasticity of demand influenced foreign consumption, as did Mun's contemporary, Rice Vaughan. But none of these writers of the early seventeenth century or their immediate successors dealt comprehensively with the relation of supply and demand.<sup>7</sup> As long as domestic trade was considered analogous to taking in each other's washing, there was no way to consider increased spending as beneficial.<sup>8</sup> Instead total demand appeared

<sup>4</sup> Sir William Petty referred to transferring wealth through taxation "from the Landed and Lazy, to the Crafty and Industrious." *A Treatise of Taxes & Contributions* (London, 1662), 19. A similar sentiment was expressed by Sir Dalby Thomas in *An Historical Account of the Rise and Growth of the West-India Colonies* (London, 1690) in *The Harleian Miscellany* (London, 1809), 2: 359. He explained that when it is said "people are the wealth of a nation, it is only meant, laborious and industrious people; and not such as are wholly unemployed, as gentry, clergy, lawyers, serving men, and beggers, etc." See also Sir Francis Brewster, *Essays on Trade and Navigation* (London, 1695), 52.

<sup>5</sup> For contemporary estimates of the Poor Law burden, see [William Carter], *England's Interest Asserted, in the Improvement of its Native Commodities* (London, 1669), 10; [Sir Humphrey Mackworth], *England's Glory* (London, 1694), 24. For a modern estimate, see Wilson, *England's Apprenticeship*, 235.

<sup>6</sup> King's figures are reproduced and analyzed in Peter Laslett, *The World We Have Lost* (New York, 1973), 36–40.

<sup>7</sup> Thomas Mun, *England's Treasure by Forraign Trade* (London, 1664), 84–86; Rice Vaughan, *A Discourse of Coin and Coinage* (London, 1675) in John R. McCulloch, ed., *A Select Collection of Scarce and Valuable Tracts on Money* (London, 1856), 82.

<sup>8</sup> For contemporary assertions that selling to one another is "mere consumption" without enrichment, see Sir Thomas Culpeper, *A Discourse, Shewing the Many Advantages Which Will Accrue to This Kingdom by the Abatement of Usury* (London, 1668), 2–3; Sir William Petty, *Political Arithmetick* (London, 1690), 82ff; John

inelastic. The rich were expected to buy their luxuries, the poor to have enough to subsist. The possibility that at all levels of society consumers might acquire new wants and find new means to enhance their purchasing power which could generate new spending and produce habits capable of destroying all traditional limits to the wealth of nations was unthought of, if not unthinkable.

During these same years that the balance-of-trade theory served as the principal explanation of economic growth, important social sentiments had become embedded in the prevailing ideas of how nations grew wealthy and powerful. Behind the balance-of-trade theory, there lay a model of the national economy which supplied the principal moral support for mercantilistic regulation. Since national wealth was believed to accrue only from the annual net gain from foreign trade, the whole economy could be conceived of as a kind of national joint-stock trading company. In this view, members of society did not interact with each other, but rather participated, one with another, in England's collective enterprise of selling surplus goods abroad. As in a company, the administration was formal. There was little of Adam Smith's awareness of individuals with personal motives working purposively on their own. Rather economic writers approached the problem of promoting national growth much as a factory foreman might view meeting a production quota. Reading through dozens of proposals for promoting English production one is forcibly struck by the absence of concern for the problem of marketing the projected increase in goods. Emphasis fell exclusively upon mobilizing labor and exploiting new resources: lowering interest rates would stimulate land improvements; attracting foreign craftsmen would introduce technical skills; agricultural diversification would relieve dependence upon outside suppliers.<sup>9</sup> With such a model at the back of their heads, these writers repeatedly elaborated schemes for putting people to work. Houses for the "orderly management of the poor" was a favorite theme. Even more indicative of the national management attitude were the frequent suggestions for a national fishery. Not only would it absorb the labor of weavers' apprentices in off-season, but one writer even suggested that the footmen of the gentry could rise early and employ their idle hours making nets, as could "disbanded soldiers, poor prisoners, widows and orphans, all poor tradesmen, artificers, and labourers, their wives, children, and servants."<sup>10</sup>

Cary, *An Essay on the State of England* (Bristol, 1695), preface; [John Pollexfen], *England and East India Inconsistent in Their Manufactures* (London, 1697), 20; *The Profit and Loss of the East-India Trade* (London, [1699]), 8-9; *Certain Considerations Relating to the Royal African Company of England* ([London], 1680), 1; Brewster, *Essays*, 50-52. In the 1690s the word "consumption" loses its pejorative connotation.

<sup>9</sup> For example, see Culpeper, *A Discourse*, 5; Samuel Fortrey, *Englands Interest and Improvement* (London, 1673) in McCulloch, ed., *A Select Collection of Early English Tracts on Commerce* (London, 1856), 234-36; *Britannia Languens* (London, 1680), in *ibid.*, 298ff; *Angliae Tutamen* (London, 1695), 29. Suviranta, *Theory of the Balance of Trade*, 153-54, errs, I think, in saying that prior to Jacob Vanderlint little attention was paid to the economic value of land.

<sup>10</sup> James Puckle, *England's Path to Wealth and Honour* (London, 1700), in Walter Scott, *A Collection of Scarce and Valuable Tracts* (London, 1814), 11: 380. See also Roger Coke, *A Detection of the Court and State of England* (London, 1694), 2: 494-95; *Reasons for a Limited Exportation of Wooll* (n.p., 1677), 18-20; Richard Haines, *England's Weal & Prosperity Proposed* (London, 1681), 6-7.

This joint-stock enterprise was a powerful image, for it provided symbolic cohesion to a society being atomized by the market. Merchants and industrialists were able to establish their place in the social order in reference to this model, and the laboring poor could find in their disciplined effort an avenue of grace. Effortlessly entwining religion with the social benefits of productivity, Slingsby Bethel railed at popular feasting because they provoked the wrath of God, wasted time, dulled wits, and made men “unfit for action and business, which is [the] chief advancer of any Government.”<sup>11</sup> Where religion failed to secure the necessary habits, laws were expected to supply the deficiency. William Sheppard urged double indemnity for those who bought wares knowing that they could not pay for them. Those who lived high, he said, should be taxed as long as they continued their excesses.<sup>12</sup> In a similar vein John Scarlett proposed discriminating among defaulters on the basis of the use made of the dissipated funds, those running into debt for riotous living being subject to the “utmost extremity” of the law.<sup>13</sup> The morality of the market was quietly fused with the morality of the marketman’s God.

The balance-of-trade theory explained how increasing exports alone could increase England’s wealth and at the same time provided a rationale for organizing labor and legislating market patterns. Associated with it were appeals to patriotism and a justification of existing economic roles. Where the theory failed was in its ability to explain English economic growth. In the closing decades of the seventeenth century, real income, domestic spending, and foreign exports rose together. From John Graunt in 1662 to William Petty in 1682 to Charles Davenant in the next decade the wealth of England drew comment.<sup>14</sup> Every index of economic growth showed an advance—agricultural output, capital investment, imports from the Indies and the New World, and the range and quantity of home manufacturing.<sup>15</sup> Most striking was the abounding evidence of a rise in domestic consumption. What had happened to the store of wealth consumed by the London fire? Contemporaries saw it splendidly replaced before their very eyes. And the rebuilding of London was but the most spectacular testimony to the fact that Englishmen were generally enjoying a higher standard of living. This growth posed questions beyond the explanatory power of mercantilist theory.

In the 1670s some writers, responding to the obvious, if uneven, economic growth, began to speculate upon the dynamic effect of increasing demand. The word “markets” in their pamphlets subtly changed from a reference to the point of sales to the more elusive concept of expandable spending. In the next decade a controversy over East Indian imports grew into a raging debate

<sup>11</sup> [Slingsby Bethel], *The Present Interest of England Stated* (London, 1671), 12–13.

<sup>12</sup> William Sheppard, *Englands Balme* (London, 1657), 147, 178.

<sup>13</sup> John Scarlett, *The Stile of Exchanges* (London, 1682), 321.

<sup>14</sup> K. G. Davies, “Joint-Stock Investment in the Later Seventeenth Century,” *Economic History Review*, 2d ser., 4 (1952), 284–85; for contemporary comment, see Petty, *Political Arithmetick*, 96–99; [Charles Davenant], *An Essay on the East-India-Trade* (London, 1696), 8–10; [William Carter], *The Great Loss and Damage to England by the Transportation of Wooll to Forreign Parts* (n.p., 1677), 12.

<sup>15</sup> Wilson, *England’s Apprenticeship*, 183; R. M. Hartwell, “Economic Growth in England before the Industrial Revolution,” *Journal of Economic History*, 29 (1969), 25; Gould, *Economic Growth*, 156–77.

on domestic consumption. According to traditional writers, the villain of the piece was the East India Company. Not only did the company enjoy a monopoly of the trade to India, but the nature of its trade—exporting bullion in return for imports competitive with English goods—ran athwart the most cherished principles of the balance-of-trade concept. From the point of view of those in the English woolen and silk industries the company's greatest crime was introducing the English public to the light, colorful, cheap fabrics of India. By 1690, the taste for chintz, calico, and muslin had reached epidemic proportions. What had begun as an inconspicuous use of cotton for suit lining had given way to a gaudy display of printed draperies, bedspreads, tapestries, shirts, and dresses.<sup>16</sup> With marketing expertise equal to Macy's, in the twentieth century, the managers of the East India Company had sent English fabric designers to India to direct the Indian craftsmen in reproducing patterns especially admired at home. The impact upon employment in England was strong. The popularity and competitive advantages of imported cottons led to a glut in the home market for woolens and silks. Contemporaries complained that thousands of workers in the two domestic industries were thrown onto the parish for support. The effected producers wanted a flat prohibition on domestic imports of Indian cottons. Their advice to the East India Company was to sell their calicoes abroad where cheap textiles would undermine the native industries of Great Britain's trade rivals.<sup>17</sup>

Although it is difficult to learn from contemporary pamphlets whether the poor were important for working up manufactures or manufacturing important for employing the poor, increasingly after 1660 the emphasis fell upon the importance of expanding opportunities for work. Bans on the export of English raw materials and proposals for replacing foreign imports with domestic substitutes were advanced on the ground that they would increase employment. From this point of view, items requiring more labor were socially more useful than those requiring less. Even labor-saving devices were suspect. The woolen and silk manufacturers drew upon this rationale in fighting the East India Company. They stressed the unfairness of searching out places which could undersell English commodities and questioned why manufacturing ought not be promoted "in England [rather] than in India."<sup>18</sup> Charles Davenant was obviously thinking within the traditional theoretical framework when he asserted that cheap imported textiles "freed" more English woolens for foreign export, but other writers recognized that the clothiers could only be convincingly answered by moving outside the balance-of-trade logic altogether.<sup>19</sup>

<sup>16</sup> P. J. Thomas, *Mercantilism and the East India Trade* (London, 1903), 30, 51; Suvaranta, *Theory of the Balance of Trade*, 7. Both Thomas and Suvaranta pointed out the stimulus of the East India trade to economic reasoning in the seventeenth century.

<sup>17</sup> *The Great Necessity and Advantage of Preserving our Own Manufactures* (London, 1697), 6-10; [Thomas Smith], *England's Danger by Indian Manufactures* (n.p., [1698]), 2-7; [Pollexfen], *England and East India*, 18-20; *Reasons Humbly Offered for the Passing of a Bill* (London, 1697), 7-23; *An Answer to the Most Material Objections* (n.p., [1699]), 1.

<sup>18</sup> *Ibid.*, 2.

<sup>19</sup> Thomas, *Mercantilism and the East India Trade*, 81.



In *Considerations on the East-India Trade*, Henry Martyn made a full frontal attack on the theory of the social utility of high labor costs by examining the differentials in domestic consumption. Conceding that Indian imports “abate the price of English Manufactures,” he maintained that this abatement stimulated other segments of the economy. Laborers who bought Indian cottons would have more money available from their wages to spend on those items produced more efficiently by the English. Even if English laborers were thrown out of work, the greater competition for jobs would lower wages and push down the cost of other English products. Driving home his cost-advantage theory, Martyn stressed that any law which forced the English to consume only English goods forced them to pay more for their needs than was necessary. He likened this to denying the benefits of new inventions or the obvious savings from the division of labor or rejecting wheat sent as a gift from God.<sup>20</sup> Martyn explored the relation between earning and purchasing power with unprecedented analytical skill. Many of his observations had been anticipated by earlier commentators. For example, Dalby Thomas had extolled the labor-saving ingenuity which the desire to acquire called forth, and John Houghton had disputed Samuel Fortrey’s strictures against French imports by pointing out that even foreign luxury items satisfied genuine consumer demands and made people work harder.<sup>21</sup> These writers legitimized domestic competition because they perceived that England was not a giant workhouse but a giant market whose individual members had differing needs.

In focusing attention on these new market relationships, the pamphlets on Indian imports revealed those areas of conflict between manufacturers and merchants which the predominating concern with foreign trade had so long obscured. Driven no doubt by self-interest, the defenders of the East India Company put forth a justification for “a good buy” which amounted to a defense of domestic consumption. Here the issue became critical to the whole structure of ideas associated with the balance-of-trade theory, because the idea of the English economy as a collective undertaking was being challenged. This line of attack cut deeper than the superficial clash of interests. The actual social atomization which came with the seventeenth-century transition to a market economy had been ameliorated by an imaginative model of economic unity organized around national production and fortified by religion and patriotism. Psychological atomization could be forestalled as long as this image retained its credibility. When individuals began to think of their separate needs and demands as acceptable social considerations, the coherence of the earlier model would disappear. The benefits of the English consumers’ having access to cheap East Indian imports depended upon the rejection of the view that society was an interlocking set of producers and distributors and the acceptance of the alternative view that the economy was

<sup>20</sup> [Henry Martyn], *Considerations on the East-India Trade* (London, 1701) in McCulloch, ed., *Early English Tracts on Commerce*, 606, 578-86.

<sup>21</sup> [Thomas], *An Historical Account*, 361-62; [John Houghton], *England’s Great Happiness* (London, 1677), 18-20.

an aggregation of self-interested individual producer-consumers. The boldest proponents of Indian imports perceived this difference and advanced a theory of economic growth based upon this perception.

When the maverick spirit of fashion revealed itself in the craze over painted calicoes the potential market power of previously unfelt wants came clearly into view. Here was a revolutionary force. Under the sway of new consuming tastes, people had spent more, and in spending more the elasticity of demand had become apparent. In this elasticity, the defenders of domestic spending discovered the propulsive power of envy, emulation, love of luxury, vanity, and vaulting ambition. On the other hand, as long as demand was viewed as more inelastic than elastic, the static conception of wealth held good. England then could only grow richer by selling a larger share of her surplus abroad, that is, by controlling a larger share of the international market. Once consumption was construed as a constructive activity, the connection could be made between progressive levels of spending or effective demand and a self-sustained momentum for economic growth. Writing in 1690, Nicholas Barbon bubbled over with the new possibilities: "The Wants of the Mind are infinite, Man naturally Aspires, and as his Mind is elevated, his Senses grow more refined, and more capable of Delight; his Desires are enlarged, and his Wants increase with his Wishes, which is for every thing that is rare, can gratifie his Senses, adorn his Body, and promote the Ease, Pleasure, and Pomp of Life."<sup>22</sup> From Dudley North came a similar expression: "The main spur to Trade, or rather to Industry and Ingenuity, is the exorbitant Appetites of Men, which they will take pains to gratifie, and so be disposed to work, when nothing else will incline them to it; for did Men content themselves with bare Necessaries, we should have a poor World."<sup>23</sup>

Less euphorically, Francis Gardner explained that while frugality was no doubt a commendable thing, "where People grow Rich. they will spend more largely, and it is better they should do so than to slacken their Industry and Diligence in Trade."<sup>24</sup> These sentiments even crept into the writings of conventional balance-of-trade writers such as John Cary, who affirmed that the growth of pride and luxury was the principal quickener of trade and extended his analysis down to "our poor in England" who can spend more on clothes and furnishings when they are paid more and hence increase the consumption of the very goods they manufacture.<sup>25</sup> An early convert to the power of consumption, John Houghton asserted that "Our High-Living is so far from Prejudicing the Nation that it enriches it." Describing the deadly

<sup>22</sup> [Nicholas Barbon], *A Discourse of Trade* (London, 1690), 13.

<sup>23</sup> [Sir Dudley North], *Discourses upon Trade* (London, 1691), 14.

<sup>24</sup> [Francis Gardner], *Some Reflections on a Pamphlet* (London, 1696 [1697]), 24, as cited in Richard C. Wiles, "The Theory of Wages in Later English Mercantilism," *Economic History Review*, 2d ser., 21 (1968), 119. Usually identified simply as "Gardner," the author was probably Alderman Francis Gardner of Norwich, who was consulted by the Privy Council on the question of recoinage, according to J. Keith Horstfield, *British Monetary Experiments 1650-1710* (Cambridge, Mass., 1960), 52, and appears among those voting against the recoinage measure in [Thomas Wagstaffe], *An Account of the Proceedings in the House of Commons* (London, 1696), 13.

<sup>25</sup> Cary, *An Essay*, 143ff.

sins as economic virtues, Houghton cited pride, finery, vanity, shows, play, luxury, eating, and drinking high as causing “more Wealth to the Kingdom, than loss to private estates.”<sup>26</sup> “Desire and want increase with riches,” Barbon observed, “a Poor Man wants a Pound; a Rich Man an Hundred.”<sup>27</sup>

Not content merely to catalogue the psychological stimulants to demand, these writers drew attention to the specific economic function of each emotion. Foreign imports were justifiable because they dazzled people with their novelty and promoted industry by way of the acquisitive instinct. Analyzing the rationale for banning foreign imports, Barbon explained that it was based on the fallacious idea that if Englishmen could not buy foreign luxuries they would consume domestic goods. This was not true, he said, because it “is not Necessity that causeth the Consumption, Nature may be Satisfied with little; but it is the wants of the Mind, Fashion, and desire of Novelties, and Things scarce, that causeth Trade.”<sup>28</sup>

Dalby Thomas made the same point when he objected to those who wanted England to live on its own without imported luxuries. They were not the source of sin, he said, but “true spurs to virtue, valour, and the elevation of the mind, as well as the just rewards of industry.”<sup>29</sup> Competition prompted men to invent things to reduce labor costs, Martyn asserted. “If my Neighbour by doing much with little labour, can sell cheap, I must contrive to sell as cheap as he.”<sup>30</sup> North described envy as a goad to industry and ingenuity even among the lowest order. When the “meaner sort” see people who have become rich they “are spurr’d up to imitate their Industry.” Even the man who goes bankrupt emulating his neighbor is a national benefactor, for the public gains from “the extraordinary Application he made, to support his Vanity.” Fashion, Barbon said, promotes trade because it “occasions the Expence of Cloaths, before the Old ones are worn out.” Rejecting sumptuary laws, North commended consumption for its stimulus to trade. Nations never thrive more than when “Riches are tost from hand to hand.”<sup>31</sup>

Behind these endorsements of early obsolescence and conspicuous consumption lay a new confidence in society’s productive powers. Where Adam Smith would use the self-sustaining power of consumption without extolling it, these writers of the 1690s actually praised prodigality. A “Conspiracy of the Rich Men to be Covetous, and not spend, would be as dangerous to a Trading State, as a Forreign War,” Barbon proclaimed.<sup>32</sup> When John Pollexfen, an unreconstructed balance-of-trade thinker on the Board of Trade, used the old moralistic arguments against luxury consumption, Gardner replied that there was “no other use of Riches, but to purchase” what served “our Necessity and

<sup>26</sup> Houghton, *A Collection of Letters* (London, 1681), 60.

<sup>27</sup> Nicolas Barbon, *A Discourse Concerning Coining the New Money Lighter* (London, 1696), 3.

<sup>28</sup> [Barbon], *A Discourse of Trade*, 72–73.

<sup>29</sup> [Thomas], *An Historical Account*, 362.

<sup>30</sup> [Martyn], *Considerations on the East-India Trade*, 590.

<sup>31</sup> [North], *Discourses upon Trade*, 15; [Barbon], *A Discourse of Trade*, 65; [North], *Discourses upon Trade*, 15. See also, *Englands Interest* (London, 1682), 6.

<sup>32</sup> [Barbon], *A Discourse of Trade*, 63.

Delight.”<sup>33</sup> The dour disapproval of self-indulgence was countered with the happy intimation of a new society of consumer-producers. “The more the merrier,” Humphrey Mackworth proclaimed, “like Bees in a Hive, and better Cheer, too.”<sup>34</sup> Moreover, the writers who promoted domestic consumption stressed the essential reciprocity of international trade. Rather precipitately labeling balance-of-trade notions as dead, the author of the Preface to North’s *Discourses Upon Trade* announced that the whole world of trade was but as one nation, concluding from this that “the loss of a Trade with one Nation, is not that only, separately considered, but so much of the Trade of the World rescinded and lost, for all is combined together.”<sup>35</sup> After asserting that either foreign or domestic consumption was good for the nation, Houghton explained in his *Letters* that import consumption enabled foreign countries to buy of England.<sup>36</sup> Henry Martyn’s cost-advantage defense of the East India Company also emphasized the mutuality of international commerce.

Accompanying the pamphlet war over Indian imports was a debate over money which pointed up the inadequacy of the mercantilist definition of wealth. According to the balance-of-trade theory, gold and silver alone were wealth, and countries without mines could become wealthy only by a carefully managed foreign trade which brought in more specie than went out. This explanation of wealth undergirt the notion of the sterility of domestic trade and led to an evaluation of all economic activities in terms of their contribution to a net balance of payments. As early as 1650, William Potter had emphasized the commodity exchange that lay at the base of commercial transactions,<sup>37</sup> but the writers of the 1690s stressed the utility of money as a means to the goods men desired. Roger Coke put it succinctly: “The wealth of every Nation consists in Goods more than Money, so much therefore as any Nation abounds more in Goods than another, so much richer is that Nation than the other, for Money is of no other use, than as employed in Trade, and the defence of the Nation.”<sup>38</sup> “To distinguish rightly in these points,” Dalby Thomas explained, “we must consider money, as the least part of the wealth of any nation, and think of it only as a scale to weigh one thing against another.”<sup>39</sup> Carrying the analysis further, Francis Gardner maintained that “some Goods are more acceptable in some Countries, at sometimes, than Money.”<sup>40</sup>

The repeated assertions that money was only a means for satisfying one’s desire for goods was but a step away from the position that consumption was the logical end of production. As Henry Martyn put it: “The true and

<sup>33</sup> [Gardner], *Some Reflections on a Pamphlet*, 7.

<sup>34</sup> [Mackworth], *England’s Glory*, 20–23.

<sup>35</sup> [North], *Discourses upon Trade*, viii. William Letwin, “The Authorship of Sir Dudley North’s ‘Discourses on Trade,’ ” *Economica*, 18 (1951), 35–45, suggests that Roger North wrote the preface to his brother’s essay.

<sup>36</sup> John Houghton, *A Collection of Letters for the Improvement of Husbandry & Trade* (London, 1681), 52–53.

<sup>37</sup> William Potter, *The Key of Wealth* (London, 1650), 2.

<sup>38</sup> Coke, *A Detection*, 2:522.

<sup>39</sup> [Thomas], *An Historical Account*, 359.

<sup>40</sup> [Gardner], *Some Reflections on a Pamphlet*, 7.

principal Riches, whether of private Persons, or of whole Nations, are Meat, and Bread, and Cloaths, and Houses, the Conveniences as well as Necessaries of Life . . . These for their own sakes, Money, because 'twill purchase these, are to be esteemed Riches; so that Bullion is only secondary and dependent, Cloaths and Manufactures are real and principal Riches.<sup>41</sup>

Speaking directly to the balance-of-trade maxim that commerce was only beneficial when more goods were exported than imported, Thomas Papillon maintained that this would only be true if gold and silver were the sole stock and riches of the kingdom: "Whereas in truth the Stock and Riches of the Kingdom, cannot properly be confined to Money, nor ought Gold and Silver to be excluded from being Merchandise, to be Traded with, as well as any other sort of Goods."<sup>42</sup> John Houghton asserted that money in coin was "good for nothing, but potentially is good for everything."<sup>43</sup>

During the debates which preceded the 1696 recoinage of England's clipped silver, attention passed to the question of whether gold and silver possessed an intrinsic and unique value which made bullion synonymous with wealth, or whether the use of money for exchange purposes made the extrinsic value derived from official minting the more important. In this clash of opinions, John Locke found himself ranged against North, Barbon, James Hodges, Henry Layton, Sir Richard Temple, and William Lowndes. Because Locke's recommendation for recoinage was based in part on the metalist view of wealth, his opponents dug away at this point. Money, Layton said, was as much a measure between items to be bartered and a commodity itself as the "natural, unalterable measure of Commodities," which Locke had made it.<sup>44</sup> Hodges said Locke's system of coin was built on the common error of considering the estimate of worth to be its intrinsic value rather than its usefulness.<sup>45</sup> Attacking Locke's belief that trade was valuable only as a means of attracting bullion, Barbon called the notion "altogether a mistake." Gold and silver were "but Commodities; and one sort of Commodity is as good as another, so it be of the same value."<sup>46</sup>

By depriving gold and silver of their unique qualities, the balance-of-trade critics opened the way for appreciating the contribution of domestic trade to economic prosperity. Daniel Defoe developed this position most thoroughly in *Taxes No Charge*. Setting forth an elaborate plan for pumping money into the economy by taxing misers and pleasure spenders, Defoe argued that if the benefit of foreign trade is to bring in commodity for commodity then that can be done at home without exposing people to the hazards of the sea.<sup>47</sup> Lowndes, Layton, and an A. Vickaris similarly drew attention to the fact that

<sup>41</sup> [Martyn], *Considerations on the East-India Trade*, 358.

<sup>42</sup> Thomas Papillon, *The East-India-Trade a Most Profitable Trade to the Kingdom* (London, 1696), 4 (originally published anonymously in 1677).

<sup>43</sup> Following this logic, Houghton, *A Collection of Letters*, 24-25, recommended bringing in goods rather than money to balance accounts, since money is unable to satisfy any real human needs.

<sup>44</sup> [Henry Layton], *Observations Concerning Money and Coin* (London, 1697), 12.

<sup>45</sup> [James Hodges], *The Present State of England as to Coin and Publick Charges* (London, 1697), 135.

<sup>46</sup> Barbon, *A Discourse Concerning Coining*, 40.

<sup>47</sup> [Daniel Defoe], *Taxes No Charge* (London, 1690), 17.



money is only uniquely prized as a means of foreign exchange, and that within the internal market the intrinsic value of gold and silver is immaterial.<sup>48</sup> Carrying the argument still further, Dudley North said that it was absurd for people to say that money was short. If there were a demand for it, it would be manufactured like anything else, since there was free coinage and plenty of gold and silver around in plate.<sup>49</sup> With increasing sophistication, these writers assessed the economic role of money. In shifting attention away from money as a store of wealth, they moved closer to recognizing the dynamic elements in the economy. The debate over money—often with the same debaters—thus re-enforced the theoretical advances made in the India imports controversy. What was needed next was to examine how the latent consuming capacity of the public at large might become an engine for sustained growth.

The idea of man as a consuming animal with boundless appetites, capable of driving the economy to new levels of prosperity, arrived with the economic literature of the 1690s. By going behind the new tastes to explore the human motives regulating personal spending, some writers discovered both a human dynamic and a market mechanism which undermined the static, specie-oriented mercantilist view. Unlike the number of working days in a person's life, energy and ingenuity organized under the stimulus of desire appeared almost limitless. Since man could satisfy his new wants only by increasing his purchasing power, what desire ultimately produced was an incentive to be more competitive in the market. From such a spring economic activity could function without outside direction. Where earlier writers had recognized the impact of taste and delight upon the market price of items, they never saw the effect of these influences upon total demand. Nor did they move to an appreciation of the role played by domestic consumption in stimulating production and total national growth.<sup>50</sup> This required a new definition of wealth and a new model of economics as a self-sustaining complex of internal relationships in which foreign trade represented accessibility to desired goods rather than the only source of riches.

The material for building a new economic theory was presented in the 1690s, but these ideas were not worked out in the succeeding decades. Instead the old formulas of the balance-of-trade theory survived with undiminished strength well into the eighteenth century. Fragments from the controversies over Indian imports can be found in the polemics over the French treaty of 1713, in the voluminous economic writings of Daniel Defoe, and in a different vein in Bernard Mandeville's *Fable of the Bees*. Clearly, to conceive of economic growth in terms of goods and services annually produced, purchased, and consumed was difficult for Englishmen long accustomed to the

<sup>48</sup> [William Lowndes], *A Report Containing an Essay for the Amendment of the Silver Coins* (London, 1695), 81–82; [Layton], *Observations*, 12–14; [A. Vickaris], *An Essay for Regulating of the Coyne* (London, 1696), 22–23.

<sup>49</sup> [North], *Discourses upon Trade*, postscript. See also [Mackworth], *England's Glory*, 5–6; Sir Richard Temple, *Some Short Remarks upon Mr. Locke's Book* (London, 1696), 4–10; John Cary, *An Essay, on the Coyne and Credit of England* (Bristol, 1696), 5–12.

<sup>50</sup> See Marian Bowley, "Some Seventeenth Century Contributions to the Theory of Value," *Economica*, 30 (1963), 122–39.

pot-of-gold image of wealth. Even after these ideas had been published, there still remained obstacles—circumstantial as well as ideological—to the idea that society was an aggregation of self-interested individuals tied to one another by the tenuous bonds of envy, exploitation, and competition.

It lies in the nature of historical investigation that developments which could be expected to take place, but do not, are rarely given the same attention as actual events. Yet a break in a development offers important clues to the nature of social change. The lacuna which needs to be explained here is the failure of writers to build upon the insights and arguments of the seventeenth-century critics of the balance-of-trade theory. Between the 1620s when Mun, Malynes, and Misselden probed the mechanism of the exchange and the 1690s when the popularity of Indian cottons and the recoinage became issues, a host of journalists, reformers, merchants, bank promoters, royal officials, London developers, members of Parliament, mathematicians, improving landlords, clothiers, and lawyers published a steady stream of tracts and treatises. Their writings paralleled revolutionary changes in the English economy: the extension of the internal market, the development of the colonial trade, the spectacular growth of London, the striking increase in agricultural productivity, the founding of the Bank of England, and the redistribution of people and production centers. As could be expected from such a motley assortment of would-be experts, the quality of the description and analysis is uneven. There is, nonetheless, an increasing sophistication in the conceptualization of wealth, commerce, and money which challenged the central premises of the mercantilist theory. The failure of others to take up this fruitful line of reasoning is the historical fact which remains unexplored.

The usual treatment of this puzzle is to appeal to the slow spread of new ideas, to minimize the amount and thoroughness of the criticism, or to refer to residual Puritan scruples against spending. Of course new ideas often spread slowly, but the rate and unevenness in intellectual currents is what we would like to understand better. The same is true of the enduring strength of old ideas such as the Puritan legacy in Restoration England. There were many beliefs and values in the complex of Puritan thought. Why did some survive into the Restoration and others not? The waning and waxing of Puritan ideals require investigation. As for the more common scholarly tendency to see the mercantilist critics of the late seventeenth century as brilliant exceptions, the range and number of their publications belie this.

The answer, I believe, is that the values embedded in the alternate theories of economic growth were incompatible with the ideological imperatives of English society. Ideology, I define as a shared and coherent view of the world which provides solidarity by assigning and rationalizing the various roles people fill in society. The moral cohesion provided by common beliefs cannot be dispensed with, particularly in a society where custom has been largely replaced by conscious decisions. In a time of profound social change, such as England was experiencing, the shared explanations which an ideology offers become all the more important because they prepare people to assume new

functions. They also facilitate the integration of new ideas with old ones. The volitional nature of belief means, however, that new explanations must satisfy a range of needs before they will be willingly accepted. The uprooting of much of the English peasantry and the geographic redistribution of workers made social control a critical issue. Ideas which explained and justified that control to both the controllers and the controlled were essential if order was to be maintained through a time of change. The new economic ideas undermined the rationale for lower-class discipline and upper-class direction. The divisiveness of competition among groups within the economy threatened a political structure already strained by clashes with the crown. Mercantilism, unlike the more intellectually impressive ideas of its critics, created morally satisfying roles in the new market society. It prescribed a path of economic development more compatible with social stability. It also deflected awareness of the tensions between economic groups within England. From this perspective, the balance-of-trade theory became economic orthodoxy in the first half of the eighteenth century not because it explained the market to contemporaries—it had ceased to do this by 1680—nor for want of better explanations, but because it offered a rationale for coercing the poor, controlling the direction of growth, and subordinating the competition among groups to the goals of economic nationalism.

Capitalism in the first part of the seventeenth century had proved compatible with traditional social stratification. The lower order became the laboring poor, while merchants, clothiers, bankers, shippers, and processors acquired the gentility formerly reserved for the landed class.<sup>51</sup> Subtle shifts in values had occurred, but the two-tiered world of the propertied and the propertyless had not been undermined. A consumption-oriented model of economic growth, on the other hand, threatened major interests of the ruling class that had coalesced in Restoration England. Dangerous leveling tendencies lurked behind the idea of personal improvement through imitative buying. The notion that the wealth of nations began with stimulating wants rather than organizing production robbed intrusive social legislation of a supporting rationale. Once it was suggested that spending in the home market was more beneficial to the economy than domestic parsimony the social benefits of statutory wage levels could be questioned. If English consumers had a right to a good buy in Indian cottons, as Martyn suggested, why could they not demand commercial policies which would protect them?

It is no accident that the men who advanced these novel opinions about domestic consumption were merchants—either outspoken defenders of the East India Company or like Dudley North, an experienced Turkey merchant, associated with foreign trades which grew with the spread of new tastes. They were not involved, as were manufacturers, in the mobilization of labor. The important variables of their commercial world were markets, prices, shipping costs, and interest rates. The manufacturers whose numbers grew with the

<sup>51</sup> Lawrence Stone, "Social Mobility in England, 1500-1700," *Past and Present*, no. 33 (1966), 52-55.

increase in industrial processing shared the clothiers' concern with employment problems. The lower class loomed larger to them as potential workers than as likely customers. In this regard, the industrial capitalists shared the interest of farmers and landed gentry, who relied upon the availability of day laborers at critical seasons of the year. Unlike the foreign merchants, these members of the ruling class had powerful reasons to maintain the credibility of the balance-of-trade explanation of national wealth. Before patriotism was the last refuge of the scoundrel, it was the valued ally of English manufacturers and landlords, creating a free labor force from the copyholders, tenants, mechanics, and craftsmen of an old order. Moreover, conventional mercantilist formulas supplied a rationale firmly based on national security for the protectionist legislation which came into full force after 1713.<sup>52</sup> To entertain the idea that "the whole World as to Trade, is but as one Nation or People" was to bring into question not only the entire Navigation System but also the wisdom of England's calculatedly aggressive national posture.

This new challenge to mercantilist thought also bore upon the more subtle problem of social control in a liberal society. Through the course of the seventeenth century, upper-class Englishmen had disentangled themselves from the constraining ties of a corporate society and embraced instead the ethos of liberalism. Slowly the individual's right to be free of inherited social obligations had gained precedence over the older notion of society's primary claim upon its members. While liberal ethics freed property and property owners from traditional social restraints, it also undermined the justification for some people's being invested with permanent authority over others. Instead, all in the society were conceived to be free and individually responsible. In arguing for this personal liberty, however, upper-class liberals had delivered most of the propertyless into the hands of a new master—the market through which they sold their labor and bought their bread. At the same time that the spirit of "possessive individualism"—to use C. B. MacPherson's insightful phrase—shattered institutional responsibility for social survival, developments in the economy separated most workers from their tools or their access to land. Without these, they were forced to sell their labor. Since only through these transactions could people feed themselves, the unseen market replaced the visible and personal authority of the previous era.

Contemporaries recognized this coercive power of the market. In 1641, Henry Robinson had defended higher food prices because the "Husbandman would hereby be brought to a frugall dyet or stirrd up to become more industrious." Likewise, farmers could "discharge a rackrent by multiplying the fruits thereof through industry."<sup>53</sup> Evaluating the effects of a poll tax, Petty adduced an added benefit from the fact that it encouraged men with many children to set them "to some profitable employment upon their very first capacity, out of the proceed whereof, to pay each childe his own Poll-

<sup>52</sup> For an interesting discussion of the repatterning of English trade under the protectionist impulse see Ralph Davis, "English Foreign Trade, 1700–1774," *Economic History Review*, 2d ser., 15 (1962), 294–95.

<sup>53</sup> Henry Robinson, *Englands Safety, in Trades Encrease* (London, 1641), 7–8.

money.”<sup>54</sup> Francis Gardner put the low wage argument in a nut shell: “The Poor, if Two Dayes work will maintain them, will not work three: and our Manufactures are never so well wrought as, in a time of dull Trade, when we pay less for Workmanship, and yet the Poor live as well then as in time of greatest Plenty, if they have but a full stroke of Work.”<sup>55</sup>

The efficacy of the market as an implement of control in a technically free economy depended, however, upon whether it was a buyer’s or a seller’s market. As John Locke commented, mechanics and apprentices lived such a “hand-to-mouth” existence that they were forced to accept food for wages rather than starve.<sup>56</sup> Another writer feared their excessive freedom and described the poor within a fifty-mile radius of London as idle and surly and willing to work only “if two days pay will keep them a week.” Others suggested an excise tax on food and drink to make the poor work a full week.<sup>57</sup> Against this background the danger of a consumption formula for economic growth can be assessed better: it threatened the most effective form of class discipline in a liberal society. The capitalistic organization of the economy could force men and women on to the labor market since that was the only place where they could earn the means of subsistence, but the amount of labor they were forced to sell depended upon the wage rate.

The employers’ need to control the labor force reveals the incompatibilities between liberalism and capitalism. Liberalism asserted the right of each person to the enjoyment of himself and the fruits of his labor. It did not matter whether laborers were predisposed toward leisure or consuming. But the economic growth which the capitalistic system promoted, required a disciplined and expandable work force. Leisure preferences could be as inhibiting to growth as archaic Poor Laws. Employers did not seek conditions of political and moral freedom for the working class. In fact, as they well perceived, transporting workers to centers of employment was much more efficient than relying upon necessitous, but free, workers to go where the jobs were.<sup>58</sup> While much has been made of the congruence between freedom and capitalism, it was the freedom of property owners from social obligations which was critical to capitalistic growth in the seventeenth century. Ideas which promoted free choice among the poor were inherently dangerous to the entrepreneurs. The writers who extolled the advantages of domestic spending in the fight over Indian imports implied that human behavior could be shaped by the impulse to consume, but such optimistic reliance upon envy and emulation must have appeared inadequate to the task of controlling laborers described as idle and surly and willing to work only if two days pay would keep them a week. Since

<sup>54</sup> [Petty], *A Treatise of Taxes*, 43. See also Thomas Manley, *Usury at Six Per Cent. Examined, and Found Unjustly Charged* (London, 1669), 24–25.

<sup>55</sup> [Gardner], *Some Reflections on a Pamphlet*, 16.

<sup>56</sup> [John Locke], *Some Considerations of the Consequences of the Lowering of Interest* (London, 1692), 34.

<sup>57</sup> *The Trade of England Revived; and the Abuses Thereof Rectified* (n.p., 1681), 8. See also E. P. Thompson, “Time, Work-Discipline, and Industrial Capitalism,” *Past and Present*, no. 38 (1967), 56–97; Keith Thomas, “Work and Leisure in Pre-Industrial Society,” *ibid.*, no. 24 (1964), 61–62.

<sup>58</sup> Sir Josiah Child, *A New Discourse of Trade* (London, 1693), 67; [Petty], *A Treatise of Taxes*, 48–49; Robinson, *Englands Safety*, 44–45.

the old social justification for ordering all economic relations had been destroyed in the fight to make property private and property-owners free, economic theory had to supply the reasons for statutory regulations.<sup>59</sup> This was the role which the balance-of-trade dogmas played in the eighteenth century.

The critical difference between Adam Smith's theory of economic growth, which was anticipated in the closing years of the seventeenth century, and that of the mercantilists was the constructive value given to consumption. This constructive value in turn rested upon the assumption that all men wished to maximize their market power because that and that alone offered an avenue of self-improvement. Although Smith's sympathies lay strongly with the savers and investors, the propensity to consume provides the linchpin for his whole system.<sup>60</sup> Externally, it creates the effective demand which calls forth production systems large enough for specialization and division of labor. Internally, the drive to truck and barter—to buy and sell—directs individual energies toward the market and away from other human satisfactions. So axiomatic is it to Smith that self-improvement will be fulfilled through economic activity that he does not even entertain ideas about alternate means to improve oneself, much less alternate goals. One improves through market power, but market power rests upon producing power. The more one produces, the more one can satisfy wants. Since self-improvement, as Smith defined it, has no natural limit, there is no limit to man's endeavors. In Smith's model, people as producers and people as consumers act like an alternating electrical current, throwing a steady flow of impulses into the economy. The flow can be taken for granted because it came from human qualities assumed to be universal.

The acceptance of the idea of universal economic rationality was the key step in the triumph of modern liberalism, because the natural economic laws depended upon natural modes of behavior. Before the laws could be accepted, the description of human nature supporting them had to be credible. Historically, however, before economic rationality became a learned pattern of response, explicit control was necessary to secure working-class discipline. The laboring poor had to be managed. The spendthrift with a feast or famine mentality had to be transformed into the shrewd saver, and the saver had to become an orderly, but compulsive, investor and consumer. Market thinking could be relied upon only after the variety of forces influencing personal preferences in the use of time and wealth had been ruthlessly narrowed to one—the likelihood of gain. This radical reductionism was the essence of

<sup>59</sup> For Edward Coke's role in endowing economic freedom with a constitutional sanction see David Little, *Religion, Order, and Law* (New York, 1969), 203–17, 243–46; Barbara Malament, "The 'Economic Liberalism' of Sir Edward Coke," *Yale Law Review*, 76 (1967), 1321–58; Donald O. Wagner, "Coke and the Rise of Economic Liberalism," *Economic History Review*, 6 (1935), 30–44.

<sup>60</sup> "Consumption is the sole end and purpose of all production; and the interest of the producer ought to be attended to, only so far as it may be necessary for promoting that of the consumer. The maxim is so perfectly self-evident, that it would be absurd to attempt to prove it." Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations* (New York, 1937) [originally published in 1776], 625. For Smith's ambivalence on the subject of consumption, compare this quotation with those on *ibid.*, 321–25.



Smith's economic rationality. The economically rational person was the one who subverted all other drives to the economic one of gaining more power in the market.

During the first part of the seventeenth century when invested capital moved England toward a market economy, only the vanguard of entrepreneurs operated as economic rationalists. To them, the rest of the society represented a problem demanding external control and direction. To meet this challenge, moreover, they relied upon a model of economic growth which obscured the possibility that the extension of economic rationalism to the working class was either desirable or possible. Rather the economic irrationality of the poor was assumed; the solution was to train them up to habits of work. When the writers in the closing decades of the seventeenth century proposed unleashing the acquisitive instincts of all classes, they were proposing a route to economic growth fraught with perils. The idea of self-improvement through spending implied genuine social mobility. The assertion that "the meaner sort" could and should emulate their betters suggested that class distinctions were based on little more than purchasing power. The moral implications of growth through popular spending were even more suspect. Unlike the work ethic which called upon powerful longings for self-discipline and purposeful activity, the ethic of consumption rested upon a moral base so shallow as to threaten the whole complex of conventional religious precepts. Calvinism had joined an ancient Christian ascetic impulse to a modern reorganization of work; the psychology of consumption offered nothing more than a calculating hedonism.

The moral anemia of appeals to consume was inextricably tied up with questions of control. Liberalism had posited man's freedom and responsibility. Capitalism required unrelenting personal effort in the market place. The two could meet only if the poor, like the rich, were converted to possessive individualism and economic rationality. Until this transition had been made, class discipline needed the support of economic theories bolstered by religion and patriotism. When capitalism and free choice were joined by Adam Smith, they were compatible because Smith could theorize from a human model in which the drive for economic self-improvement predominated. This conception of man was the antithesis of freedom, for it presumed a compulsive market response. In recommending the democratization of consumption in the 1690s, the proponents of a spending model of economic growth were revealing for the first time the tensions that lay beneath the values and sensibilities associated with the producing and consuming sides of capitalism. Only when economic rationalism had become internalized by the working, as well as the investing, class could liberal economics support the onus of its amorality. The ideology of mercantilism in the meantime blunted the force of new ideas.

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# The Classical Theory of Deference

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J. G. A. POCOCK

A DEFERENTIAL SOCIETY IN THE CLASSICAL—that is, eighteenth-century English and American—sense is usually conceived of as consisting of an elite and a nonelite, in which the nonelite regard the elite, without too much resentment, as being of a superior status and culture to their own, and consider elite leadership in political matters to be something normal and natural. Whether elite leadership means simply that leaders must come from the elite, or, in addition, that the leadership given on specific issues by members of the elite is normally to be followed—by no means the same thing—is not perfectly clear. But this summary of deference as an ideal type is intended to suggest that it contains another ambiguity which renders it both convenient and problematic as a tool for the historian. Deference is expected to be spontaneously exhibited rather than enforced. A slave or a serf is flogged into obedience, not deference, and the deferential man is frequently depicted as displaying deference as part of his otherwise free political behavior. He defers to his superiors because he takes their superiority for granted, as part of the order of things. It is often suggested that what makes him do so is the conditioning effect of tradition and that the deferential society is closely akin to another favorite conceptual tool of historical sociologists—the traditional society.

Deference is the product of a conditioned freedom, and those who display it freely accept an inferior, nonelite, or follower role in a society hierarchically structured. Scholars who employ the concept of a deferential society, on either side of the Atlantic and of the year 1800, however, also pay considerable attention to something less subjective, which Castlereagh characteristically termed “persuasion in a tangible shape”: that is, to the notion of an influence, reaching from pure deference toward inducement and even coercion, and including such means of social control as agrarian tenancy and political patronage, which the elite in the deferential society exercised over their inferiors. And it is supposed that finally there occurred—in America during the 1780s or 1820s, in Britain during the 1830s or 1860s—a democratic rebellion

This paper, which serves as an introduction to the two following articles, was part of a symposium on deference presented at the 1974 meetings of the American Historical Association in Chicago. Richard W. Davis’ paper, now revised for publication, was also read at that session. David Spring, who was not part of the Chicago program, wrote his paper subsequently to round out this published symposium.

against deference and influence alike, a movement toward a world in which there was no god but equality and Tocqueville was his prophet.

This straw man, if such it be, is woven of a thick and complicated texture, permitting some ambiguities and confusions. It is perhaps time to look at a few ways in which the notion of deference has been and may be applied, not with any specially destructive intentions toward its use as an analytical tool but in the hope of clearing up some of the misunderstandings that may have attended its use. Clearly, the understandings of British Whigs and American Federalists need not be the same as those of their twentieth-century interpreters, but the two views are likely to illuminate one another. In seeking to understand the so-called deferential society, we should start by inquiring what that society thought deference was.

Deference is an oldish word, whereas the term “deferential society” is a twentieth-century neologism. And though political speakers and writers of the eighteenth century knew the word and used it on occasion, they did not employ it as a key concept or as a means of denoting an essential attribute of political society. They were acquainted, however, with what may be considered the essential meaning of the term: the voluntary acceptance of a leadership elite by persons not belonging to that elite, but sufficiently free as political actors to render deference not only a voluntary but also a political act. As clear and simple a description of this effect as any appears in James Harrington’s *Oceana*.<sup>1</sup> We are invited to suppose that out of any twenty men engaged in political decision, six will be of superior capacity to the others. Harrington’s emphasis falls, however, less on the superior capacity of the six than on the recognition of this capacity by the fourteen. He emphasizes repeatedly that this will be instant, unforced, and infallible. One would say that he presents it as rational but for the difficulty of applying that term to the recognition of a higher rationality by a lower. At all events, there is no need to restrain or compel the fourteen in their identification, recognition, and choice of the six. The six will be there, and the fourteen will find them. The fourteen will acknowledge the superior capacity of the few and accord them the authority of fathers, less in the sense in which Filmer’s patriarchs enjoy a God-given *patria potestas* than in that in which Roman senators were called *patres conscripti*. But the fathers owe their authority less to their own superiority than to the acknowledgement—it would be proper to call it election—of their inferiors. Here, surely, is what is meant by deference.

Harrington’s fourteen are, no less than the six, active citizens charged with performing political functions. The first and most important of these is the finding and recognition of the six. But since Harrington was a republican rather than a parliamentarian, he did not incur the wrath of Rousseau by implying that the fourteen would then go home and leave the six in plenipotentary enjoyment of their mandate. A complex distribution of functions ensued between the six and the fourteen, which to Harrington—indiffer-

<sup>1</sup> John Toland, ed., *The Oceana and Other Works of James Harrington* (London, 1771), 44, 236–38.

ent as he was to separation of powers in the eighteenth-century sense—reduced itself to the distinction between “debate” and “result.” It was for the six to conceive and initiate policies, to articulate the differences between them, to argue the cases for and against each one. When the debate of the aristocracy was at an end, however, it was for the democracy—without speech or argument, perhaps by the silent routines of Venetian balloting—to determine which of the policies or courses proposed should actually be adopted. Harrington stresses not only that liberty is at an end when debate and result are lodged in the same hands, but actually that the nonelite fourteen are better fitted to exercise the final determination than the elite six.<sup>2</sup> The superior capacities which distinguish the six fathers turn out to be capacities for invention, articulation, foresight, and analysis—the virtues of *theoria*. When it comes to actual decision, something else counts. Perhaps it is experience, and the fourteen have more of it because there are more of them. Alternatively, it is honesty, and the fourteen-six relation insures it in both parties. At all events, though the many acknowledge the few to be superior in their capacities, the relation between debate and result is one of equality. Deference, then, is perfectly compatible with equality, so long as the latter is proportionate equality in the Aristotelian sense. Indeed, this sort of equality cannot exist unless qualitative distinctions and inequalities among men are recognized. And if Harrington’s “debate” and “result” be equated with the “speaking aristocracy” and “silent democracy” of which we hear in seventeenth-century New England,<sup>3</sup> it becomes apparent that the “silent democracy” need no more lack political will and power than need the “silent majority.”

Harrington was writing in this way because he felt the need to rehabilitate aristocracy in the wake of what he saw as the collapse of feudal oligarchy. When a few owned the land and the many were their tenants, the latter were subject to the power of the former and lacked political capacity. But when this state of affairs collapsed, and the many acquired both propertied independence and political capacity, there necessarily appeared a republic in which the democratic component was of vast importance. It was self-evident, however, to any republican theorist that the people in a commonwealth must be differentiated into an aristocratic and a democratic component; but according to Harrington’s theory of deference, they divide naturally, voluntarily, and spontaneously. There is little need to legislate the special qualifications defining an aristocracy. In the six of superior and recognized capability, we detect the “natural aristocracy” (of so much concern to John Adams), whose progressive collapse from the 1780s to the 1820s provided American political culture with its first prolonged internal crisis. Yet if a theory of deference was coterminous with a theory of natural aristocracy, it was a way of arguing that

<sup>2</sup> *Ibid.* 44, 236–38, 487.

<sup>3</sup> The phrase is Samuel Stone’s. See also Perry Miller, *The New England Mind* (Boston, 1961), 452. James Harrington would have said that if Stone allowed the congregation no “result,” he had no business using the word “democracy.”

no other aristocracy was possible or necessary; and it was a way of welcoming, legitimating, and liberating the newly acquired political capacities of the many. It furnished the grounds, we may suspect, on which Senator Barry Goldwater, centuries later, has felt able to welcome the extension of the franchise to eighteen-year-olds.

The superior capacities of the six will not consist of attributes of personality alone. These talents will also be recognized through outward economic and cultural signs—wealth and birth, leisure and property, liberality and education. Harrington was a gentleman, who thought politics had something in it peculiarly suited to the genius of a gentleman,<sup>4</sup> and he sought to reassure his class that they need not fear for their leadership status in a yeoman commonwealth. But though he was assuring other gentlemen that the material foundations of their elite status would remain intact, his point was that their property and culture would be recognized by the *demos* as part of the superior natural capacities which the *demos* also recognized. The link between personal capacity and material circumstance—in Harrington's language, between the goods of the mind and the goods of fortune<sup>5</sup>—was provided by the Aristotelian theory of leisure. Property brings leisure, the opportunity to turn the mind away from property and toward the common good of which it is part. Harrington's six will fairly certainly have more property than the fourteen—more leisure and opportunity to develop superior capacity—and the capacity for the limited sort of leadership which is expressed in debate as compared with result. But what keeps it a limited leadership is the fact that the fourteen too possess property, and therefore leisure and the ability to know something about the common good. Their political capacity is not confined to recognizing and choosing the six; retaining the power of result, they retain the capacity of evaluating the policies which the six have the capacity to propose.

We are now in the world of Charles Sydnor's gentleman freeholders, the idealized Virginia of *The Candidates*.<sup>6</sup> There the yeomen are certainly deferential in the sense that they accept leadership by a natural aristocracy. They recognize extrapersonal characteristics such as birth, wealth, and culture—the members of the gentry seem obliged to emphasize their education and their libraries—as outward and visible signs of the superior personal capacities they are looking for. But the yeomen must first find and evaluate their superiors. They are presumed capable not only of knowing a fake natural aristocrat when they see one, but also of asking sensible and pertinent questions of the genuine article. Deference precludes them from the capacity for leadership, but not from an intelligently critical attitude toward those who possess that capacity. It is wholly compatible with proportionate equality and public virtue. Indeed, if these things presuppose political relations between individuals of diversified capacity, they depend upon deference, and deference

<sup>4</sup> Toland, *Oceana*, 53.

<sup>5</sup> *Ibid.*, 36, 41–2.

<sup>6</sup> Charles S. Sydnor, *Gentlemen Freeholders: Political Practices in Washington's Virginia* (Chapel Hill, 1952).

is no more than the recognition of one capacity by another. It might even be shown by the few toward the many.

Moderns are disposed to think, however, that if one man has more money than another he has power over him, or at any rate power that he does not share with him; and they feel that Harrington's equation of power with property ought to have included this perception. Harrington, however, did not think in this way; he was too exclusively concerned with feudal tenure. He thought the House of Lords had existed because the nobility had once been a feudal baronage. Since that no longer existed, there could no longer be a House of Lords. The political relations between Englishmen could only be those obtaining between classically equal citizens, of which the deference that institutionalized a natural aristocracy was an important part. But several things happened in 1660 that he had failed to foresee. While the House of Lords was restored, feudal authority was not. Yet the House of Lords flourished for a long time thereafter. In considering the obvious social power of the peerage, historians may conclude that the lords had ways of maintaining social power, and of retaining men in dependence, which Harrington had failed to consider. But was that necessarily how the matter appeared to contemporaries?

There was widespread acceptance of that part of the Harringtonian interpretation which stressed that since the landholding classes were no longer divided into barons and their dependent vassals or retainers, England must now be governed by a scheme of civic relations obtaining among its independent proprietors. The latter, according to classical and constitutional theory, must be divided into aristocratic and democratic components, and there now existed a hereditary but no longer feudal upper house to play the aristocratic role. Harrington and other radicals of the Interregnum thought it important to insure that the feudal nobility was not replaced by any other kind of "standing aristocracy,"<sup>7</sup> whether of hereditary officeholders or of elect saints. The Restoration House of Lords, however, claimed no hereditary monopoly of any significant political function; the lords had instead hereditary titles, hereditary rights of summons to parliament, and lands inherited indeed, but inherited in much the same way as by any other gentleman. Edmund Burke, like most thinkers of the eighteenth century, argued that in a polity of independent proprietors, the lords' hereditary dignity and hereditary role in parliament rendered them independent in a very special degree. This is the point of his famous and not really sycophantic letter to Richmond, in which he says that the hereditary peers are like great trees and the new men—like Burke himself—the none too hardy annuals that bloom in their shelter.<sup>8</sup> All men are independent, Burke is saying, and the hereditarily independent are not different from the rest of us. They are those animals who are more equal than others.

<sup>7</sup> For example, see Marchmont Nedham, *The Excellencie of a Free-State* (London, 1656).

<sup>8</sup> Lucy S. Sutherland, ed., *The Correspondence of Edmund Burke* (Cambridge and Chicago, 1960), II, 377.



The theory of aristocracy in eighteenth-century England rested upon the assertion that a hereditary aristocracy, so long as it did not reduce all others to dependence, was perfectly capable of acting as a natural aristocracy. The House of Lords could be praised for possessing the virtues of such an aristocracy; we read this as late as 1867, in Coventry Patmore's informative if nauseating poem about "the year of the great crime/When the false English nobles and their Jew,/By God demented, slew/The Trust they stood twice pledged to keep from wrong."<sup>9</sup> Patmore ascribed to the nobility before their apostasy such things as dignity, leisure, *sprezzatura*, the unbought grace of life, and the Aristotelian virtues generally, and indicated that a class endowed with these virtues had incomprehensibly renounced a political leadership which might still have been rewarded with deference had they continued to affirm it. But while Harrington's natural aristocracy received deference within a political process, in Hanoverian England deference operated as often as not outside electoral procedures and even in such a way as to render them unnecessary. If deference was, then, a concept so highly civic as to be quasi-republican, what was its theoretical role in an increasingly oligarchical society, where the political activity of the lesser proprietors tended for at least two generations to be progressively reduced?

There was always an alternative, in both theory and practice, to deference as the voluntary respect which one kind of political capacity paid to another. This alternative was the influence or patronage—possible only in a world of office—which government might exercise over society, or patrons over clients. Most historians would agree that this had a great deal to do with the survival of the peerage in a shape which Harrington had failed to predict, and those who wish to reconcile the "crisis of the aristocracy," culminating in 1640, with its spectacular revival in the century beginning about 1660, seem disposed to stress that members of the nobility growing up under the restored Stuarts were involved in government to the point where they might well have become an official aristocracy rather than a class of parliamentary magnates.<sup>10</sup> But the restoration of the House of Lords was part of the restoration of the parliamentary constitution, and the rhetoric of that restoration stressed the lords' role as a *pouvoir intermédiaire*, a "screen or bank," as the phrase ran in English, pointing toward their role as the hereditary natural aristocracy of the land-owning and propertied nation. After 1688 and again after 1714, the peerage did develop into a class of parliamentary magnates—or a dominant component of that class—but at the same time it becomes less and less necessary to distinguish between the influence which peers exercised by reason of their territorial holdings, the influence which they exerted over parliamentary elections and electorates, and the influence which they possessed because of their activity in government, office, and departments of state. If from one point of view England seemed a vast Country, composed of independent

<sup>9</sup> Coventry Patmore, *Poems*: vol IV: *The Unknown Eros* (London, 1879), 56.

<sup>10</sup> John R. Western, *Monarchy and Revolution: The English State in the 1680s* (London, 1972).

proprietors of various sorts of freehold, there was another from which it appeared a vast and dispersed Court, in which everyone constantly sought patronage from those in positions to give it.

Here is one of the fundamental ambiguities of our subject, that between “deference” and “influence.” No doubt there was a deference proper to the relations between client and patron: the sort of deference which Burke displayed toward Richmond, or Hutchinson toward Hillsborough,<sup>11</sup> or Chatham—for that matter—toward George III. But in these three cases (and many others), the client was deeply concerned to maintain his independence. Deference was not merely his means of inducing the patron to grant what he had to offer, but also the means of reminding the patron that it was not his business to reduce the client to dependence in the sense of servility, but to treat him in a way which acknowledged the independence and self-respect of both parties. The technical term for this was “affability.” Only a great man could have affable manners. It was a failure of *ton* to ascribe them to one not of superior station, but a patron who failed in affability toward Jonathan Swift or Samuel Johnson was likely to carry scar tissue to remind him that his station had its duties.

There were those who realized that the patron-client relationship, in the form it took when the latter was a man of honor and independence, had much in common with the relationship between lord and vassal, and this doubtless contributed to the idealization of feudal society observable toward the end of the eighteenth century. But once it was admitted—it was sometimes denied—that the lord had been able to oblige and compel the vassal to follow him, then it was no less certain to the Whig mind that eighteenth-century Britain was a postfeudal society, and that deference and influence were equally techniques of social control necessary because society was in such a state. Neither could operate, or would be necessary, except among independent men. But it seemed abundantly clear to theorists that deference could not corrupt men or reduce them to servility, because in the last analysis it was concerned with what Harrington had called “the goods of the mind.” Conversely, influence in the sense of patronage could corrupt, because it was concerned with persuasion in a tangible shape, with the material and social rewards which Harrington had ranked among “the goods of fortune.” There were thus moral tensions between deference and influence, which it was the business of social morality, in the age of great expectations, to try to overcome; but to go any further in this direction would be to enter that fascinating and difficult territory where the two conceptions interpenetrate.

Given that in the half-century or longer beginning about 1780 there occurred in Britain and America extensive reorganization of both electoral institutions and electoral behavior, which can be illuminated by applying the concept of deference to them, the model constructed here, by extrapolation from Whig political perceptions, suggests that in Britain the continued and

<sup>11</sup> Bernard Bailyn, *The Ordeal of Thomas Hutchinson* (Cambridge, Mass., 1974).

massive importance of the peerage was explained and legitimized by the contention that a natural aristocracy, expecting deference, and a hereditary aristocracy, exerting influence, were not mutually exclusive but could, within limits, be identical. In America, however, the relative unimportance of hereditary station and political patronage meant that political elites, where they existed, were, above all in the postindependence era, cast more exclusively in the role of natural aristocracy and compelled to rely upon the expectation of deference to the exclusion of any other means of maintaining their status. Both societies were postfeudal, but one had been directly modified by Whig parliamentarism in ways that the other had not. This explains why American political thought soon became and long remained preoccupied with acclaiming or deploring the failure of natural aristocracy, and the Federalists with the role of aristocrats struggling to understand the failure of the deference due them. Likewise, it dramatizes, if it does not fully explain, the fact that America preceded Britain in experiencing the rise of equality in a revolutionary, Tocquevillian sense in which established elite characteristics were considered irrelevant to claiming political leadership.

Richard W. Davis examines how far Whig parliamentary reform can be explained as an attempt to enlarge deference at the expense of influence, by increasing the independent at the expense of the dependent electorates, and in what sense, if any, the term deference itself is useful as a description of the ways in which independent electorates actually behaved. It would be an ironical conclusion if we found that democratization in Britain was, even initially, a successful experiment in the perpetuation of deference, since it was in America that the rise of democratic machine-politics led to the assumption by influence and patronage of an importance they had never had before and cannot be said to have lost since. In this respect, new democrat was but old Whig writ large, and it would be strange to find that our two societies had exchanged the roles assigned them by the eighteenth-century antithesis. Recent work by several historians of America has drawn attention to the Federalist perception of a democratic assault upon classical education as part of the overthrow of deference and virtue.<sup>12</sup> In Britain, classical education was brilliantly revived by radical Tories like the Arnold family as part of a not unsuccessful attempt to bring a Coleridgean meritocracy into being to redress the balance upset by the decay of the propertied aristocracy. There were utopian dimensions to this experiment, as the younger Arnolds discovered—Matthew by writing about barbarians, philistines, and populace, Thomas junior by emigrating to seek deference as a clerical leader in such unpromising places as New Zealand and Tasmania. But in Britain the creation of a mandarin state proceeded, in however un-utopian a form. Perhaps this inquiry into deference should conclude by asking at what points an advanced education became a positive disadvantage in the pursuit of elective office. And perhaps part of the answer would be that in Britain it has not altogether happened, even yet.

<sup>12</sup> For example, see Linda K. Kerber, *Federalists in Dissent* (Ithaca, 1970).

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# Walter Bagehot and Deference

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DAVID SPRING

UNTIL RECENTLY THE REPUTATION of Walter Bagehot has stood high with students of nineteenth-century England. Because he was a man of many parts, not only historians, but also students of literature, politics, and economics have admired him. Many of them, if asked, would probably have agreed with that master of Victorian studies, G. M. Young, that Bagehot was their favorite Victorian. Bagehot wrote well. He was a maker of memorable phrases, a man also of sympathy, taste, learning, and wit. If, as Young has said, he was not "the greatest man alive and working between 1837 and 1901," so much the better. Bagehot was "a man not too illustrious or too consummate to be companionable, but one, nevertheless, whose ideas took root and are still bearing."<sup>1</sup> In short, Bagehot was easy to like, and to such a man posterity tends to be kind, restraining the critical faculty it exercises so readily upon less likeable and more eminent men.<sup>2</sup>

Not that critical voices have never been heard. Maynard Keynes early in this century noted that Bagehot did not always reach the level of his reputation. His economics was better psychology than it was economics. He could theorize to a limited degree in familiar fields of observation. But he was sometimes tempted beyond his powers. He could become entangled in "a train of inference," as Keynes noted, that was "too long for him or for which, not truly caring," he had no "steady patience." Similarly, even his gift for psychological insight was limited. It was good when exercised upon "those of a middle position," primarily "business men, financiers, and politicians." It was less good when exercised upon either the high or the low. "Genius and violent feeling on the one hand," wrote Keynes, "and servitude, confinement, and distress upon the other" were beyond him.<sup>3</sup>

During the last decade, however, voices critical of Bagehot have grown more numerous. For the most part the critics have been political scientists.<sup>4</sup> But

<sup>1</sup> George M. Young, *Today and Yesterday* (London, 1948), 240.

<sup>2</sup> See Alan Ryan, "Bagehot's Britain," *Times Literary Supplement*, Dec. 27, 1974, 1453-54.

<sup>3</sup> John Maynard Keynes, "The Works of Bagehot," *Economic Journal*, 25 (Sept. 1915), 369-70, 372.

<sup>4</sup> The criticism by political scientists probably began with Maurice J. C. Vile, *Constitutionalism and the Separation of Powers* (Oxford, 1967). Vile was primarily interested in Bagehot's constitutional theory, which he was the first to attack: "Not only was his claim to originality [on the cabinet] false, but his treatment of the central principles of the Constitution reveals a distorted and unhistorical approach to the subject he

even historians—who as a rule are more cautious, slower to leap on or off bandwagons—have begun to join in the criticism.<sup>5</sup> “It has long been clear,” observed Olive Anderson, “that we need both a serious historical appraisal of Bagehot’s ideas and a serious attempt to explain Bagehot’s sustained posthumous renown.”<sup>6</sup>

Of course, I intend nothing so ambitious in this short essay. Rather, I intend to analyze his social and political thought to the degree necessary to illuminate his concept of deference. To be sure, deference was a key concept for Bagehot. More than anyone else Bagehot brought this idea into current intellectual vogue. Geoffrey Best recently called Bagehot the Darwin of deference.<sup>7</sup> And more than anything else in his writing, deference has caught the interest of historians. I suspect, however, that they have not read him as carefully as they might have. It may clear the air therefore to re-examine Bagehot’s social and political thought to see what he meant by deference and to see if his ideas of deference are trustworthy guides to nineteenth-century English political life.

Bagehot dealt with deference most explicitly in *The English Constitution*, and historians have usually derived their ideas of it from this book. A better place to begin, however, is *Physics and Politics*, a much less glittering and quotable book, but one containing the underlying theory on which *The English Constitution* stands. In *Physics and Politics* Bagehot, the Victorian social evolutionist, described the long development of society and politics as did Maine and Spencer in terms of polar opposites. This is a characteristic strategy of Bagehot in all his writings—to deal with reality as a dualism. This strategy partly accounts for the seductive quality of his work. “Bagehot’s attraction,” V. S. Pritchett wrote, “is that he speaks in our accent; he has our drastic habit of mind, our analytical approach.” Nothing is more drastic than Bagehot’s fondness, as Pritchett put it, “for clearing the ground by cutting it in two and then contrasting the two parts.”<sup>8</sup> There are thus two kinds of novel, two kinds of men, two kinds of cosmology, two kinds of education, and—most relevant for our purposes—two kinds of political society. This social and political duality Bagehot summed up as tradition versus reason, or the “stationary” versus the “progressive” forms of civilization. A stationary state, he believed,

claimed to lay bare for the first time” (p. 214). After Vile, came attacks by Dennis Kavanagh, “The Deferential English: A Comparative Critique,” *Government and Opposition*, 6 (Summer 1971), 333–360; and “An American Science of British Politics,” *Political Studies*, 22 (Sept. 1974), 251–70. Most recent, and perhaps most notable of all, is Samuel Beer, “Tradition and Nationality: A Classic Revisited,” *American Political Science Review*, 68 (Sept. 1974), 1292–95. Unfortunately, I have not been able to use for this essay the recent British Academy lecture by K. C. Wheare on Walter Bagehot.

<sup>5</sup> Among historians should be included James P. Cornford, who is also a political scientist. See his review of Bob Jessop, *Traditionalism, Conservatism, and British Political Culture in Times Higher Education Supplement*, Oct. 4, 1974, 15; and of Robert T. McKenzie and Allan Silver, *Angels in Marble in History*, 58 (Feb. 1973), 127–9. See also Richard W. Davis, “The Whigs and the Idea of Electoral Deference: Some Further Thoughts on the Great Reform Act,” *Durham University Journal*, 67 (Dec. 1974).

<sup>6</sup> See Olive Anderson’s review of Charles H. Sisson, *The Case of Walter Bagehot in History*, 58 (June 1973), 312.

<sup>7</sup> Geoffrey Best, *Mid-Victorian Britain 1851–1875* (London, 1971), 236.

<sup>8</sup> Victor S. Pritchett, “Books in General,” *New Statesman and Nation*, 28 (July 29, 1944), 74.

“is by far the most frequent condition of man, as far as history describes that condition; the progressive state is only a rare and an occasional exception.”<sup>9</sup>

Bagehot considered at length the modes of coherence, the organizing principles of these opposing societies. Stability and order preoccupied him. “How to get the obedience of men,” he wrote, “is the hard problem.”<sup>10</sup> While in his twenties, he observed in his *Letters on the French Coup d’État of 1851*: “The first duty of society is the preservation of society. . . . Legislative Assemblies, leading articles, essay eloquence—such are good—very good—useful—very useful. Yet they can be done without. . . . Not so with all things. The selling of figs, the cobbling of shoes, the manufacturing of nails—these are the essence of life.”<sup>11</sup> Not many young men of Bagehot’s social background talked this way. That Bagehot did so may well have been rooted in personal circumstance. He had grown up in close quarters with the insanity of his mother and half-brother. The recurrent threat of family chaos was an early and terrifying reality, which in all likelihood informed his later experience of society and politics.

Bagehot found the stability and order of traditional society to stem from the human propensity to imitate. This he characterized as both instinctive and unconscious, as something not rooted in the intellect. “At first,” he maintained, “a sort of ‘chance predominance’ made a model, and then invincible attraction, the necessity which rules all but the strongest men to imitate what is before their eyes, and to be what they are expected to be, moulded men by that model.”<sup>12</sup> Thus man took the first steps in the general endeavor of “nation making.” Thus came to being the “cake of custom,” that voluntary, automatic self-discipline, that “hereditary drill,” by which the mass of a nation deferred to and followed its leaders.<sup>13</sup>

Bagehot took much satisfaction in enunciating the mimetic principle, which he found at work in Victorian no less than in primitive society. Time after time he gave it emphatic expression. On one occasion he declared he believed “unconscious imitation to be the principal force in the making of national characters.”<sup>14</sup> On another occasion he wrote that “the propensity of man to imitate what is before him is one of the strongest parts of his nature”;<sup>15</sup> and on still another, that “this unconscious imitation and encouragement of appreciated character . . . is the main force which moulds and fashions men in society as we now see it.”<sup>16</sup> Was this the Darwinian social scientist speaking, pleased with his anthropological discovery? Or was it the frightened Victorian gentleman seeking assurance that society really was stable and orderly? Probably both.

Despite his fears of social disorder, Bagehot was ambivalent about tradi-

<sup>9</sup> Mrs. Russell Barrington, ed., *The Works and Life of Walter Bagehot* (London, 1915), 8: 137.

<sup>10</sup> *Ibid.*, 8: 17.

<sup>11</sup> *Ibid.*, 1: 84, 137.

<sup>12</sup> *Ibid.*, 8: 24.

<sup>13</sup> *Ibid.*, 8: 18.

<sup>14</sup> *Ibid.*, 8: 25.

<sup>15</sup> *Ibid.*, 8: 60.

<sup>16</sup> *Ibid.*, 8: 63.



tional society. Its dreary uniformity, its childlike men and women, above all its lack of freedom distressed the Victorian liberal in Bagehot. He sometimes spoke of traditional society, indeed, as if it were valuable mainly as a basis upon which a progressive one might be built. "The ages of monotony had their use," he wrote, "for they trained men for ages when they need not be monotonous."<sup>17</sup>

The unmonotonous age—the age that came so rarely—was one of variety, progress, and, above all, reason. Its origins were more obscure than those of traditional society. But somehow the society of mere order in some few places did the hard thing: it got out of "a fixed law," broke the "cake of custom." This began "in states where the government was to a great and a growing extent a government by discussion, and where the subjects of that discussion were in some degree abstract, or, as we should say, matters of principle."<sup>18</sup> The habit of discussion, once established, brought in its train the cherished goods of a progressive society—freedom, choice, tolerance, science, and invention.

The age of free discussion prompted an even more obvious display of Bagehot's ambivalence. On the one hand, he wrote glowingly of its virtues. Liberty was "the strengthening and developing power—the light and heat of political nature";<sup>19</sup> and in England, he declared proudly, there was "on the whole probably a freer discussion of a greater number of subjects than ever was before in the world."<sup>20</sup> On the other hand, Bagehot also entertained a quite different vision of liberty, a fearful vision that it undermined the basis of order: "As soon as discussion begins," he wrote, "the savage propensities of men break forth; even in modern communities, where those propensities, too, have been weakened by ages of culture, and repressed by ages of obedience, as soon as a vital topic for discussion is well started the keenest and most violent passions break forth. Easily destroyed as are early free states by forces from without, they are even more liable to destruction by forces from within."<sup>21</sup>

This was an odd view of man and society, at once humane and inhumane, bold and timid. It was dualistic through and through. The good society needed both tradition and reason, custom and discussion. Without the one there was disorder, without the other sterility. But the two principles, as Bagehot understood them, were at bottom incompatible. Order forbade liberty and liberty order; "discussion and custom," he argued, "cannot be thus combined; their 'method,' as modern philosophers would say, is antagonistic."<sup>22</sup> This may explain why in *The English Constitution*—Bagehot's famous account of politics and society in the age of Palmerston—he brought them together as co-existent but wholly separate parts of the English state, their separation presumably guaranteeing that they not destroy one another, and

<sup>17</sup> *Ibid.*, 8: 20.

<sup>18</sup> *Ibid.*, 8: 102.

<sup>19</sup> *Ibid.*, 8: 143–4.

<sup>20</sup> *Ibid.*, 8: 106.

<sup>21</sup> *Ibid.*, 8: 117.

<sup>22</sup> *Ibid.*, 8: 112.

their coexistence within the same constitutional framework assuring that they somehow support each other. But why, apart from the imperatives of Bagehot's anxious fantasies, need order and liberty, custom and discussion, be wholly separate entities? Bagehot never attempted to answer this question. *The English Constitution*, by far the most memorable of his writings, is thus a curious document. Contradiction and unreality mar its brilliance.

*The English Constitution*, wherein Bagehot explicitly deals with deference, stands on the same duality as *Physics and Politics*. What Bagehot in one work called tradition and reason in the other he called respectively the dignified and efficient parts of the constitution. This famous dichotomy, to use Bagehot's phrase, was a scheme of "double government."<sup>23</sup> In such a government, he argued, "there are two parts . . . : first, those which excite and preserve the reverence of the population—the *dignified* parts, if I may so call them; and next, the *efficient* parts—those by which it, in fact, works and rules . . . ; every constitution must first gain authority, and then use authority."<sup>24</sup> Although Bagehot hastened to add parenthetically that the dignified and efficient parts are "not indeed separable with microscopic accuracy,"<sup>25</sup> and although he sometimes let slip a perfunctory reminder that constitutional bodies like the House of Commons or the House of Lords had both parts in them, it was the idea of separation that he constantly reiterated. Certainly this is the idea that his readers come away with.

The efficient parts of the constitution were the House of Commons and the cabinet (the latter incidentally a body Bagehot claimed he had discovered);<sup>26</sup> the dignified parts were the monarchy and the House of Lords. The business of the dignified parts of the constitution was to provide those "theatrical elements" in political life that inspired obedience, or deference, in the mass of English society. "The office of an order of nobility," Bagehot explained, "is to impose on the common people . . . , to impose on their quiescent imaginations what would not otherwise be there."<sup>27</sup> The conduct of efficient business was for the most part beyond the nobility. "The qualities which fit a man for marked eminence, in a deliberative assembly, are not hereditary and are not coupled with great estates."<sup>28</sup> "It is as great a difficulty to learn business in a palace," he said, "as it is to learn agriculture in a park."<sup>29</sup>

Thus, the landed elite and the royal family functioned chiefly as prompters of deference—to such a degree, indeed, that England was (to use a phrase coined by Bagehot) a "deferential community."<sup>30</sup> Characteristically, Bagehot's deferential community contained "crowds of people scarcely more civilized than the majority of two thousand years ago"<sup>31</sup>—or, as he put it even

<sup>23</sup> Walter Bagehot, *The English Constitution* (World's Classics edition, London, 1928), 255.

<sup>24</sup> *Ibid.*, 3–4.

<sup>25</sup> *Ibid.*, 3.

<sup>26</sup> For Bagehot's false claim to originality, see Vile, *Constitutionalism and the Separation of Powers*, 213–219.

<sup>27</sup> Bagehot, *English Constitution*, 79.

<sup>28</sup> *Ibid.*, 84.

<sup>29</sup> *Ibid.*, 106.

<sup>30</sup> *Ibid.*, 239.

<sup>31</sup> *Ibid.*, 5–6.

more brutally, a deferential community was one “in which primitive barbarism lay as a recognized basis to acquired civilization.”<sup>32</sup> Having, as he added, “whole classes unable to comprehend the idea of a constitution—unable to feel the least attachment to impersonal laws,” such a community was fortunate in being able to provide at one and the same time both “a comprehensible element for the vacant many” and “complex laws and notions for the inquiring few.”<sup>33</sup> The latter flourished because the efficient part of the constitution—that government by discussion without which, according to Bagehot, “no State can be first-rate”<sup>34</sup>—found shelter from the depredations of the vacant many (whom Bagehot feared to enfranchise) behind the façade of a theatrical, deference-generating display.

Bagehot’s idea of deference was something other than the Harringtonian theory described by J. G. A. Pocock. To be sure it was a deference that a non-elite spontaneously offered to an elite. The members of the non-elite were not virtuous republicans, however, but that new nineteenth-century collectivity—vast, mysterious, intimidating—known as the masses, who on the whole were devoid of property and education. Accordingly, although Bagehot’s deference was freely given, there was little of the intellect in it, at least in the form which Bagehot most often, if not invariably, put forward. His deference was much like the mimesis of traditional society in his *Physics and Politics*: “As yet the few rule by their hold, not over the reason of the multitude, but over their imaginations and their habits”;<sup>35</sup> or again, “the mass of the English people . . . defer to what we may call the *theatrical show* of society. . . . What impresses men is not mind, but the result of mind.”<sup>36</sup> Deference so conceived, not surprisingly, got Bagehot into difficulties.

How well did Bagehot’s leading ideas on deference fit the facts of nineteenth-century English political life? Samuel Beer, not mincing words, contended that Bagehot was blind “to elementary facts of political behavior and of British political experience.”<sup>37</sup> Bagehot’s principal idea, the separation of the traditional and the progressive, the dignified and the efficient, was simply not to be found in practice. The landed elite, said Beer, was at once traditional and progressive. For Bagehot, however, the landed elite seemed little more than an antiquated adornment to the working machinery of government, not unlike those “solely-ornamental wheels introduced into the clocks of the Middle Ages, which tell the then age of the moon or the supreme constellation;—which make little men or birds come out and in theatrically.”<sup>38</sup> This was hardly an apt figure for one of the most effective ruling elites in world history. Admittedly, like all elites, it had its share of mediocrity and inertia, but on the whole England’s was vigorous and businesslike,

<sup>32</sup> *Ibid.*, 34.

<sup>33</sup> *Ibid.*

<sup>34</sup> *Ibid.*, 311.

<sup>35</sup> *Ibid.*, 239.

<sup>36</sup> *Ibid.*, 236–7.

<sup>37</sup> Beer, “Tradition and Nationality,” 1292. My debt to Samuel Beer is large. He was the first to see the close relation between *The English Constitution* and *Physics and Politics*.

<sup>38</sup> Bagehot, *English Constitution*, 186.

leaving its mark on all the major departments of English life. What Bagehot overlooked was its obvious propensity for leadership.

If we accept Bagehot's notion of the landed elite as a maker of theatrical display and of little else, then his non-elite becomes merely the passive victim of a giant confidence trick. But few in his own day, the day of Reform Bills and Corn Law agitation, saw class relations in this way. Thus Matthew Arnold, no less intelligent than Bagehot, found the common people "deferential"—his word—but it was a deference that responded to spirited and humane leadership and emanated from ordinary men who were themselves vigorous, high spirited, and full of "a self-reliance which disposed each man to act individually and independently."<sup>39</sup> In short, Arnold's was the deference of people who knew where they were being led. They were capable of throwing off their deference, as Arnold thought ordinary Englishmen were doing by 1861, if their customary leaders should no longer provide adequate leadership.

Arnold stood on firmer ground than Bagehot. The idea of deference that Bagehot commonly put forward left little room for the possibility that the lower classes could make a rational and independent choice, little room for the influence of public opinion. Is it too much to say that more went on in Victorian political life than instinctive deference, than the mere registering of the finest brute voters in the constituencies in support of the finest brute votes in the House of Commons?<sup>40</sup> Bagehot himself was too good an observer to rest content with so misleading an account. On occasion he saw things differently. This Bagehot is not as visible as the first. But neither is he hard to find.

Indeed Bagehot has much more to say on public opinion than might be expected from the Darwin of deference. After all, he was also the eulogist of cabinet government, which made for systematic parliamentary discussion, for the "great scene of debate, the great engine of popular instruction and political controversy . . . , the best means yet known for arousing, enlivening, and teaching a people."<sup>41</sup> Presumably, this instruction caught on, for Bagehot next wrote of "the opinion out of doors, the secret pervading disposition of society" that had a "great influence" on how members of Parliament voted. "The nation feels," he wrote, "that its judgment is important, and it strives to judge."<sup>42</sup> Perhaps, however, Bagehot had in mind some narrow and superior part of the nation, not its wide and inferior part. But later in *The English Constitution*, the reader comes across the phrase "the ordinary intelligent many"<sup>43</sup> (having earlier encountered the "vacant many"), and still later he finds the statement that a "great many ideas, a great many feelings, have gathered among the town artisans—a peculiar intellectual life has sprung up among them."<sup>44</sup>

<sup>39</sup> P. J. Keating, ed., *Matthew Arnold: Selected Prose* (Harmondsworth, 1970), 108.

<sup>40</sup> Bagehot, *English Constitution*, 125. "A cynical politician is said to have watched the long row of county members, so fresh and respectable-looking and muttered, 'By Jove, they are the finest brute votes in Europe!'"

<sup>41</sup> *Ibid.*, 17.

<sup>42</sup> *Ibid.*, 18–19.

<sup>43</sup> *Ibid.*, 73.

<sup>44</sup> *Ibid.*, 153–4.

Not surprisingly, the Bagehot of “public opinion” created problems for the Bagehot of “deference.” In one of his last essays, “Lord Althorp and the Reform Act of 1832,” a new kind of deference appeared that Bagehot called “intellectual deference.”<sup>45</sup> Presumably, this was a deference rather like Arnold’s, grounded less on instinct than on intellect. According to Bagehot, “Intellectual deference used to be paid to members of Parliament,” but the reformers of 1832 “destroyed intellectual constituencies in great numbers without creating any new ones.”<sup>46</sup> Despite its apparent death in 1832, intellectual deference was still alive for Bagehot in the 1870s. In his introduction to the second edition of *The English Constitution* he grudgingly conceded that English voters, however deferential, exercised their intelligence to the extent of choosing between two rich candidates, although he belittled this choice as merely deciding on an issue “selected by the higher classes.”<sup>47</sup> A few pages later, however, he confessed that “in excited states of the public mind,”<sup>48</sup> the higher classes were not free to select the issue. Then, a few pages further, the Bagehot of “public opinion” would seem to have virtually routed the Bagehot of “deference.” Whereas in the first edition of *The English Constitution*, Bagehot had written amusingly on the fondness of the English voter for baronets and earls (“a manufacturer’s son has no chance” against them),<sup>49</sup> he now wrote that Englishmen individually liked a lord, but when they acted in concert—as in elections—they easily found themselves committed to “anti-aristocratic sentiments . . . , and their collective action may be bitterly hostile to rank.”<sup>50</sup>

Clearly there are dangers in using Bagehot as a guide to English political life in the nineteenth century. If there is a Bagehot of “deference,” there is also a Bagehot of “public opinion,” and equally disconcerting, the Bagehot of “deference” does not speak with one voice. Whatever the voice, however, it is an immensely seductive one, consciously employed by a brilliant journalist who, as he said, coveted “power, influence over people’s wills, faculties, and conduct more in proportion than I can quite defend.”<sup>51</sup> These he gained, both in his own day and ours, but it may be time to resist his blandishments. Historians might begin by using the word “deference” with greater care—at least until something more comes of recent inquiries into its meaning,<sup>52</sup> and something more is done to investigate the behavior of Victorian voters.

<sup>45</sup> Barrington, *Works*, 7: 69.

<sup>46</sup> *Ibid.*, 69–70.

<sup>47</sup> Bagehot, *English Constitution*, 265.

<sup>48</sup> *Ibid.*, 270.

<sup>49</sup> *Ibid.*, 148.

<sup>50</sup> *Ibid.*, 278.

<sup>51</sup> Quoted in Norman St. John-Stevan, *Walter Bagehot* (London, 1959) p. 25.

<sup>52</sup> Among the sociologists, the work of Howard Newby is notable. See “The Deferential Dialectic,” *Comparative Studies in Society and History*, 17 (April 1975), 139–164. Newby’s sociology of deference is a distinct improvement on Edward Shils, “Deference,” in J. A. Jackson, ed., *Social Stratification* (Cambridge, 1960), 104–132. Among political scientists, the work of Bob Jessop is notable. See his *Traditionalism, Conservatism, and British Political Culture* (London, 1974), especially chapters 2 and 7.

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## Deference and Aristocracy in the Time of the Great Reform Act

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RICHARD W. DAVIS

A THEORY OF DEFERENCE has recently played a significant role in modifying older explanations of the intention and effects of the Great Reform Act. The historian chiefly responsible for the new interpretation is D. C. Moore, who insists on "the electoral importance of what might be called the 'deference community,' the community of men who lived in close contact with one another, who had the same occupation or were connected by the same 'interest,' and—most important of all—who recognized the same individual, or individuals, as their social, economic, and ideological leader or leaders." These deference communities, Moore contends, were all-important and all-pervasive. Every elector was a member of one or another community, and the great bulk of the electorate tamely followed the preferences of the relatively few leaders. Moore goes so far as to assert: "For conceptual purposes, of course, deferential behaviour should be distinguished from behaviour motivated by economic, social, or religious interest. In practice, however, in most constituencies for most of the century this distinction cannot be made."<sup>1</sup>

Moore argues that Lord Grey and his colleagues in the reform ministry were aware of this phenomenon and built their measure upon it. They had no intention of increasing the electoral power of the middle classes or of giving the individual elector a real voice. Rather, they sought to strengthen the influence of the leaders in the several deference communities, most particularly the influence of the larger landowners, the class that had always dominated English politics. The Reform Act was intended, then, not as a concession, but as a cure.<sup>2</sup>

John Cannon and Michael Brock have argued convincingly, however, for

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<sup>1</sup> D. C. Moore, "Social Structure, Political Structure, and Public Opinion in Mid-Victorian England," in Robert Robson, ed., *Ideas and Institutions of Victorian Britain* (New York, 1967), 36, 47.

<sup>2</sup> D. C. Moore, "The Other Face of Reform," *Victorian Studies*, 5 (1961), 7-34; D. C. Moore, "Concession or Cure: The Sociological Premises of the First Reform Act," *The Historical Journal*, 9 (1969), 39-59. Moore's arguments will doubtless be developed further in his forthcoming book. In the meantime, the author must acknowledge a private correspondence that has been most helpful in clarifying our differences, even if it has not removed them.



the traditional view of the Reform Act as a concession. But not altogether consistently, they continue to accept Moore's notion of electoral deference, or something very like it. Cannon accepts the notion as applied to counties and smaller boroughs, and he agrees—quite rightly—with Moore in stressing the continuing weight of such constituencies in the electoral system.<sup>3</sup> And Brock, in attempting to explain away the apparent implications of the Whig emphasis on a propertied and independent electorate, writes that the Whig view “by which these apparently conflicting principles were reconciled was that the right kind of deference flourished best among men of some property and education.” “Such men,” he says, “deferred to the local magnate in the only way that was still entirely unobjectionable: they accepted his advice and leadership in politics, not because they had no alternative, but because they knew enough to recognize his superior knowledge. The borough electorate of £10 householders could thus be held to combine all the virtues, the voters being at once propertied, deferential, and independent.”<sup>4</sup>

The theory of deference advanced in the last few years as an explanation of the Great Reform Act, whether right or wrong, is new. It obviously bears little relation to the classical theory discussed by J. G. A. Pocock, for that left the many free to choose between the men and the measures of the few. As David Spring makes clear, Walter Bagehot, who did so much to popularize the notion of deference in the nineteenth century, with an influence lasting to the present day, was in fact confused—and hence confusing—on the question. His writings really reflect little interest in, or understanding of, the electoral process. But Bagehot was at least occasionally drawn toward the concept of what he called “intellectual deference” in which the electorate exercised a rational and independent choice in selecting between their upper-class leaders. No one would doubt that England had a deferential society in the sense that the mass of the electors voted for their betters, but this is not at all the same thing as voting as their betters told them to. Not until recently has it been argued that deference implies the obedient casting of a vote according to the preference of a social superior.

Not only is modern theory largely without precedent, but also it contradicts the privately and publicly expressed opinions of the act's framers. Contrary to Moore's contention that they did not distinguish between influence and public opinion, they made a very clear distinction. Writing to Lord Palmerston in October 1831, Grey identified what he believed comprised public opinion and distinguished it from the most prevalent form of influence when he spoke of “the middle classes, who form the real and efficient mass of public opinion, and without whom the power of the gentry is nothing. . . .”<sup>5</sup> He told Maurice Fitzgerald, the Knight of Kerry, that the times required that “a greater influence may be beneficially exerted upon the government by im-

<sup>3</sup> John Cannon, *Parliamentary Reform, 1640–1832* (Cambridge, 1973), 246–7, 255–6.

<sup>4</sup> Michael Brock, *The Great Reform Act* (London, 1973), 143.

<sup>5</sup> Grey to Palmerston, Oct. 10, 1831, Broadlands Manuscripts GC/GR/2042/1–3. The author is grateful for the permission of the Trustees of the Broadlands Archives to make use of the papers at the National Register of Archives.

provements in the representative system. . . .”<sup>6</sup> Advising his son, Lord Howick, on a speech to his new Northumberland constituents in May 1831, Grey said that it was the aim of the reform bill “to give the People an efficient voice in the conduct of their own affairs, and to provide the best security both for the Crown and the other Branch of the Legislature, by connecting their interests with those of the Publick.”<sup>7</sup> Lord Melbourne took up a similar theme in October, extolling to the king’s secretary, Sir Herbert Taylor, “the advantages derived from popular institutions . . . , which mainly consist in the contentment, acquiescence, and satisfaction derived from the consciousness felt by the people that they either watch over their own interests, or have each of them an equal voice in nominating those whom they trust with that duty.”<sup>8</sup> And Lord Lansdowne, like Melbourne one of the more cautious reformers in the cabinet, never contemplated a plan that would not satisfy the middle classes and, as he wrote to Grey in January 1831, “give persons of small property a decidedly larger share of power.”<sup>9</sup> These statements do not square with the intentions, ascribed by recent historians to the ministers, of a merely passive role for the mass of the electorate.

There is ample evidence that Grey and his colleagues did not subscribe to such simple and all-embracing explanations of electoral behavior as those suggested in the modern theory of a deferential electorate. Writing to Taylor in November 1831 Grey argued that “by giving a second representative to the great towns you will ensure the return of one of the master manufacturers and people of property in the towns; whereas if there was only one, there might be an apprehension that he would be returned by the class of operatives. . . .”<sup>10</sup> Clearly Grey was apprehensive that the operatives might not behave deferentially, but rather vote according to their own inclinations. To encourage compromise the more usual situation of two members and two votes for each elector was to be introduced.

This device was based on experience, and not only or even primarily on experience with large urban electorates. As Lord Althorp remarked during the debates of the summer, every gentleman who had ever been engaged in a county contest knew that “there were always private and personal considerations which induced electors to give their second vote in a different manner to that in which they had given their first. . . .”<sup>11</sup> There would be no place in the historians’ deferential electorate for “private and personal considerations.” The framers of the Reform Act made allowance for such considerations because they knew they had to.

<sup>6</sup> Grey to the Knight of Kerry, n.d., Grey Papers (Department of Palaeography and Diplomatic, University of Durham).

<sup>7</sup> Grey to Howick, May 6, 1831, *ibid.*

<sup>8</sup> Lloyd C. Sanders, ed., *Lord Melbourne’s Papers* (London, 1971), 136.

<sup>9</sup> Lansdowne to Grey, Jan. 18, 1831, Grey Papers.

<sup>10</sup> Grey to Taylor, Nov. 30, 1831, RA Add 15/1618 (Royal Archives, Windsor Castle). See also Henry, Earl Grey, ed., *The Correspondence of the late Earl Grey with H. M. King William IV* (London, 1867), I, 454. The author was able to make use of the material in the Royal Archives by gracious permission of Her Majesty Queen Elizabeth II.

<sup>11</sup> *Hansard’s Parliamentary Debates*, 3rd series, V, 1371.

The framers of the act also knew, of course, that electors would continue to be exposed to various pressures and also to what might be called coercive, as opposed to moral or intellectual, influence. But they clearly intended to minimize the effects of such pressure and influence. Furthermore, the intent to create a deferential electorate, in the sense in which that term has been recently used, cannot realistically be attributed to men such as Grey. They could hardly have expected to create an electorate that merely reflected the opinions of its social superiors, or if they did, they went about it in a very strange way.

The most significant evidence of the ministers' intention was their first plan, drawn up in December 1830 by a committee chaired by Lord Durham and composed of Lord John Russell, Sir James Graham, and Lord Duncannon, and accepted with some modifications by the cabinet in January 1831. The £20 franchise for boroughs was the most significant element in the proposal. The usual explanation of this high franchise is that it was a concession extorted from Durham in return for an agreement by his colleagues on the committee to support the ballot. But all the explanations at the time have just the reverse emphasis. Durham himself suggested to Brougham that "if you conceded the Ballot in the first instance you might easily obtain (so great is the desire for the Ballot) a much higher qualification, as the price of it, which would assure you an independent and excellent constituency."<sup>12</sup>

Grey told Taylor that it was precisely this argument that had been used in the cabinet. The ballot, Grey said, "was not proposed even by the framers of the report, as one to which they were themselves partial, but as a concession which would greatly facilitate the raising of the elective franchise, in Cities and Boroughs. . . ." Grey went on to explain that he, Brougham, and Lansdowne among others had argued strongly against the ballot. It had been rejected, though, he said, some still thought it a necessary concession to secure the acceptance of the higher franchise, which all apparently desired and which was retained.<sup>13</sup> Grey also intimated to the king that he would have been willing to accept the ballot had it been the only way to get the high franchise.<sup>14</sup> Finally, there is Russell's testimony as to the framers' intentions and priorities. Writing to Durham on February 13, 1831, he said: "When we agreed to make ballot a part of the plan to be proposed to Lord Grey, we did so on the ground that such a concession to popular feeling would enable us to vest the elective franchise in a body above corruption, and deeply concerned, by reason of their property, in preserving our mixed Constitution."<sup>15</sup> Russell wrote to Durham in great consternation. He had just discovered that a £20 franchise would create minute electorates in many smaller boroughs and, he said, make the government a laughing stock. The ministers then decided to revert to the £10 franchise, which had been the previous proposal before the committee.

<sup>12</sup> Durham to Brougham, Dec. 30, 1830, Brougham Papers (University College, London).

<sup>13</sup> Grey to Taylor, Feb. 8, 1831, RA Add 15/1288. See also *Corresp.* I, 114.

<sup>14</sup> Grey to William IV, Feb. 5, 1831, RA Add 15/1283. See also *Corresp.* I, 106.

<sup>15</sup> Russell to Durham, Feb. 13, 1831, Grey Papers.

The Whigs' mastery of detail left something to be desired, and too much cannot be made of the facts and figures they threw about with such reckless abandon. But some idea of what they envisioned for most boroughs from the higher franchise may be gained from Russell's remark to Grey that householders of £7 to £8 in small boroughs were "equal to £20 in large towns."<sup>16</sup> The ministers desired and intended to create a very select franchise in the majority of boroughs. It is hardly likely that they would have expected the resulting electorate to be deferential. Rather, there would have been every reason to anticipate, in Durham's words, "an independent and excellent constituency"; or, as Russell said, to believe that the franchise would be vested "in a body above corruption, and deeply concerned, by reason of their property, in preserving our mixed Constitution."

Talk of a mixed constitution and independent electors suggests an explanation of the Reform Act that has never had sufficient attention. We have been accustomed to regard the framers of the act as proto-democrats, as pragmatic conservatives, or more recently as possessed of a distinctive sociological theory. But Russell, speaking of the first plan, suggested a different interpretation. The plan contained, he said, "hardly anything new. Lord Grey and other reformers had repeatedly since the year 1780 drawn the great outlines of Parliamentary Reform."<sup>17</sup> Historians have been too inclined to see the debate in the 1830s as isolated and arising almost solely out of the conditions and necessities of that period, largely ignoring the eighteenth-century roots of the question. But, as Russell, who was steeped in the thought of the earlier period, reminds us, Grey was an eighteenth-century reformer. His ideas had been formed before what is conventionally dubbed the Industrial Revolution and the social and economic changes that accompanied it had begun to make much impact on men's minds, and before they talked much about the middle classes. Though Grey's notions had been modified and adapted to new conditions and issues, basically they had changed very little.

The usual contrast drawn between a radical young Grey and a conservative old one is misleading. Undoubtedly, much of what he had advocated in his previous reform proposals of 1797, he contended against as dangerous radicalism in the 1830s. Household suffrage, roughly equal constituencies, triennial parliaments, and single-member constituencies are outstanding examples. But these were specifics, the means to implement the ends. And in the earlier period, as in the later, Whig reformers tended to be much sounder on principles than they were on details. If one turns one's attention to the arguments and principles of the 1797 proposals, one will find little that would have been out of place in the 1830s.

No more in the earlier period than in the later did the Whig advocates of reform believe that it would tend toward democracy. Grey assured the House of Commons in 1797 that "he wished to alter no part of the constitution." "It was his desire," he said, "that it should remain as it had been established,

<sup>16</sup> Russell to Grey, May 23, 1831, *ibid.*

<sup>17</sup> Russell to Durham, Oct. 19, 1834, Russell Papers, British Museum Add Ms 38080 ff 73-8.

composed of King, Lords, and Commons.” Not only did he wish to maintain a mixed and balanced constitution, he also foresaw no fundamental change in the social composition of the House of Commons. Under his plan, the landowner “would find his property suitably represented; the merchant would find support in the householders; and men of respectability and talents in the different professions, would find a fair door open for getting into parliament. The only persons whom he would wish to exclude from that House, were men who were neither possessed of landed property, nor engaged in commercial enterprise, nor professors of any particular science; but men who, without property, without industry, and without talents, obtained seats in the House of Commons by the influence of great men, for the purpose . . . , not of consulting for the good of the people, but of promoting their own interests.”<sup>18</sup> This was Grey’s stand in 1797.

It is instructive to set against this Grey’s advice to Howick in 1831. The reform bill, he said, was of a “conservative nature.” It would give “the People an efficient voice in the conduct of their own affairs” and “provide the best security both for the Crown and the other Branch of the Legislature by connecting their interests with those of the Public.” The bill, said Grey, would take nothing from the higher orders that they ought to have. The influence of “high station and of property” would remain if those who enjoyed those advantages showed “by their conduct a claim to the confidence and good opinion of their fellow subjects.” But the wealthy and the powerful would “no longer have the power of nominating members, either for money, which in the case of the freeholder would be called a bribe, or for personal advantages to Members or their families.” Nor would they “retain the power of dictating to the electors.” These not only formed “no part of the constitution,” but were in “direct opposition to its fundamental principles,” which established “the right of *representation* and not of *nomination*. . . .”<sup>19</sup>

Both arguments demonstrate the same concern for a balanced constitution and a common desire to rid the constitution of its corruptions and give the electorate a positive role. Fundamental to both arguments is a belief that the electorate will recognize its natural leaders, and that they will be worthy of being so recognized. Indeed the basic arguments in the two periods are indistinguishable. The old “Friend of the People” had not put away the ideas of his youth.

The notion of the independent electorate had a similar source in the earlier debates, though in 1797 it was Charles James Fox rather than Grey who had enlarged upon the idea and defined “the most perfect system” as that “which shall include the greatest number of independent electors, and exclude the greatest number of those who are necessarily by their condition dependent.”<sup>20</sup> As it was reckoned that the householder franchise which Grey proposed in 1797 would produce only 600,000 electors as opposed to 3,000,000 under

<sup>18</sup> William Cobbett, *Parliamentary History*, 33 (London, 1818), 650.

<sup>19</sup> Grey to Howick, May 6, 1831, Grey Papers.

<sup>20</sup> Cobbett, *Parliamentary History*, 33, 727.

universal suffrage, clearly the earlier franchise (as well as the later) was intended to be highly selective.<sup>21</sup>

For the framers of the Reform Act, it was not difficult to identify the middle classes with the independent electorate which was the ideal of eighteenth-century reformers. The Whigs endowed both with the same virtues of independence, excellence, and intelligence. Not that they had a precise definition of middle class in mind. Indeed, theirs was hardly a definition at all, referring pretty literally to all those who lay in the middle sections of society, between the greater landowning classes on the one hand, and the unskilled manual laborers on the other. Certainly the middle classes were not associated in their minds exclusively with urban and industrial areas. That association is a later one, and it has long confused discussions of the Great Reform Act. What Grey once called “the middle and more influencing classes”<sup>22</sup> existed in Buckinghamshire as well as Birmingham, in Wiltshire as well as Westminster; and everywhere their property, intelligence, and independence made them worthy candidates for the franchise, and for a greater power in the political system. Whether the manufacturing or mercantile interest should predominate over the landed interest was seen by everyone as a crucial question—but it was a different question. The Whigs never dismissed rural and agricultural England as subservient. That is a later notion.

The Whigs intended to introduce a genuine political life throughout the system. Because they did not see the two intentions as incompatible, they also contended that they and their kind should continue to lead. Arguing for the enfranchisement of Manchester and Birmingham in 1821, Russell clarified not only the Whig attitude on the benefits of extending the representation, but also on the roles and relationships of candidates and their constituents.<sup>23</sup>

In neither of these rich and populous communities were there individuals to whom, from their rank or official station, the people were accustomed to look for the tone and colour of their political opinions. In towns that were represented, however violent the politics of those towns might be, there were certain persons candidates for seats in that House, who acted under the control of public opinion, and who gave a consistent colour to the opinions of the body whom they wished to represent. A popular election, besides giving vent to discontent, embodied the vague wishes of hostile parties, and forced all to seek some object which had at least a plausible and legal appearance. It was to the want of any such political centre in these towns, that he was inclined to attribute some of these unfortunate occurrences which had taken place amongst them since the war. There was no authority to which they could conform, or from which they could derive instruction.

Candidates and electors were seen as mutually influencing. Candidates would educate and moderate, give tone and color, but always within limits set by public opinion. It was a genuine political process the Whigs had in mind. They did not confuse political leadership with ideological control of the electorate.

<sup>21</sup> *Ibid.*

<sup>22</sup> Grey to Taylor, Jan. 18, 1832, RA Add 15/1687. See also *Corresp.* II, 137.

<sup>23</sup> *Hansard's Parliamentary Debates*, New Series, V, 615. The author is grateful to Harold Ellis for drawing this quotation to his attention.



The strong Whig aversion to the ballot, probably shared by all the ministers save for Durham and Althorp, may seem to argue against such a view. Yet the Whig dislike of the ballot and preference for open voting must be viewed in the context of the whole elaborate system of checks and balances in which eighteenth-century, and thus Whig, thinking delighted. William IV, in remarks which Grey said had the sympathy and concurrence of his ministers, objected to the ballot because he said it would be “a protection to concealment, would abolish the influence of fear and shame, and would be inconsistent with the manly spirit and the free avowal of opinion which distinguish the people of England.”<sup>24</sup> Russell believed that it would lead to “bribery and profligacy.”<sup>25</sup> These remarks do not necessarily suggest anything sinister. So long as the vote was seen not as a right, but as a public trust, open voting was viewed as another means of ensuring that an elector would act with deliberation and responsibility; secret voting, as Russell suggests, was disliked because most Whigs believed that it would lead to corrupt and self-interested behavior. But Whig aversion to the ballot must be seen in context, and their evident concern for electoral independence set against it. Their dislike of the enfranchisement of tenants-at-will in the counties by the Chandos Clause was genuine, their aversion to coercion by landlords real.<sup>26</sup> Yet no more in the elector than in the Member of Parliament did they wish to encourage irresponsibility. As the M.P. had often to weigh other loyalties and convictions against the displeasure of his constituents, so the elector should be exposed to a play of influences. It was the strong, however, not the weak, who should feel its pressures.

Whig ideology gave a large role to the people, by which they meant men of independent opinions, the middle classes. Whig ideology and Whig experience also gave the leading role to the aristocracy. They did not believe that the entrenched influence of the unreformed system was necessary to preserve that leading role to the aristocracy. Of course they recognized the influence of property. Of course they hoped that their own dependents would defer to their wishes. But in the final analysis the Whigs put their faith elsewhere. As Lord Holland said before the first elections under the act, they had “only to find good Candidates and the people will chuse them.”<sup>27</sup>

<sup>24</sup> William IV to Grey, Feb. 4, 1831, RA Add 15/1279. See also *Corresp.* I, 97.

<sup>25</sup> Russell to Durham, Oct. 19, 1834, Russell Papers, BM Add Ms 38080 ff 73–8.

<sup>26</sup> See, for example, Grey to Durham, Aug. 28, 1831, Grey Papers; Melbourne to Brougham, Sep. 20, 1832, Brougham Papers; Russell to Palmerston, Sep. 23, 1832, Broadlands Manuscripts GC/RU/4; Grey to Palmerston, Sep. 21, 1832, *ibid.* GC/GR/2147/2.

<sup>27</sup> Holland to Grey, Jun. 11, 1832, Grey Papers.

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# On Decolonization and Informal Empire

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ROBIN W. WINKS

TO DECOLONIZE AN EMPIRE usually entails one of three processes.<sup>1</sup> If the imperial masters believe firmly in the doctrine of preparation, with its notions of trusteeship or stewardship, then they must assume a series of measurable, clearly defined steps through which a colonial society passes until it may be deemed ready for independence, a readiness to be judged in part by the colonizer and in part by the colonized, but seldom in equal measure between them. Since the doctrine of preparation implies clear stages of progress toward the stated goal of resuming those rights held only temporarily in trusteeship by an imperial power, the historian should be able to judge—perhaps only dimly at the time but more clearly with hindsight—where in the scale of readiness the colony stands. To be sure, since the imperial power defined the stages through which a colony must pass, and in large measure was the only effective judge of where amidst those stages the colony stood at any given moment, the imperial nation controlled the process to the end. After all, it and not the colony had designed the ladder to freedom, had defined each rung upon it, and could tell the colonial—by invoking emergency powers, or suspending a constitution here and there—how soon he might contemplate taking the next step. If the colonial seemed unclear as to the lesson he had learned, he might be asked to go through one or more of the steps again. As Tom Mboya remarked: “Efficiency is the last refuge of the imperialist.”<sup>2</sup>

This first, gradualist form of decolonization carried with it a strong assumption that the appropriate means by which progress toward the top rung on the ladder was best measured lay in the political sphere, ordinarily (in the British Empire) through progressive transfer to the colony of the Westminster model

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<sup>1</sup> It will be obvious to readers knowledgeable in the field of imperial studies that this paper owes a large intellectual debt to the fundamental work of John Gallagher, Eric Stokes, Ronald Robinson, and D. A. Low.

<sup>2</sup> The author draws here, and elsewhere, upon his own essay, *Failed Federations: Decolonization and the British Empire* (Nottingham, 1971), Cust Foundation Lecture.

of Parliament. One result of this assumption is that those who held to the doctrine of preparation saw decolonization first in political terms and only then in economic, social, or intellectual ones—that is, to one-half of an idealist conception of culture. That there was a catch to the process was clear enough: rioting or corruption (as defined by the West) on the part of the indigenous society would be taken as evidence that the process had been carried out too quickly, justifying slowing or stopping the process entirely.<sup>3</sup>

The indigenous elites saw the two ironies inherent in this situation soon enough. The first was that many former colonies became independent—that is, received back their full rights after the period of trusteeship—only by accepting independence upon imperial terms. At the very moment of birth the new nation was, in the eyes of many subgroups within it, fatally compromised; its form of government was not autochthonous,<sup>4</sup> its independence being a gift rather than an assertion, its very nature Europeanized. For where independence comes by evolution, rather than by revolution, as even Canada learned, the nation's definitions of self, its nationalism, would be of a nature often radically different from other nationalisms. The second of the ironies followed from the first, from the necessity on the part of the indigenous leadership to accept formal decolonization according to a theory designed for the colony from outside. Zenobia, in Boris Vian's play *The Empire Builders*, asked the question: "... if there's no staircase any longer, after we've moved up once again," where are we to go?<sup>5</sup> What is one to do when the ladder of success no longer exists, having been carted away along with other now extraneous carpentry by the master craftsmen as they departed? In fine, nationalist leaders of a colony found that they had one set of goals as they worked toward independence, and that these had to be exchanged almost overnight for another set of goals as those leaders became (assuming that they succeeded in doing so) the inheritors of power. Developmental goals differed from custodial goals, and accordingly so did the tactics for economic or further political disengagement.<sup>6</sup>

Now it must be clear that when a historian looks at the decolonization of a so-called informal empire, the process must seem even less clearly defined than in the case of evolutionary independence from a formal condition of

<sup>3</sup> Of a large literature, see in particular Frederick Madden, "Some Origins and Purposes in the Formation of British Colonial Government," in Kenneth Robinson and Madden, eds., *Essays in Imperial Government, Presented to Margery Perham* (Oxford, 1963), 1–22; Robinson, *The Dilemmas of Trusteeship* (London, 1965); Ged Martin, *The Durham Report & British Policy* (Cambridge, 1972); D. B. Swinfen, *Imperial Control of Colonial Legislation, 1813–1865* (Oxford, 1970); Geoffrey Bolton, *Britain's Legacy Overseas* (Oxford, 1973); and Francis G. Hutchins, *The Illusion of Permanence: British Imperialism in India* (Princeton, 1967).

<sup>4</sup> See Kenneth C. Wheare, *The Constitutional Structure of the Commonwealth* (Oxford, 1966).

<sup>5</sup> See also Aimé Césaire, *Discourse on Colonialism* (New York, 1972).

<sup>6</sup> Richard Symonds, *The British and Their Successors* (London, 1966); Brian Crozier, *The Morning After: A Study of Independence* (Oxford, 1963); William P. Kirkman, *Unscrambling an Empire: A Critique of British Colonial Policy, 1956–1966* (London, 1966); William B. Cohen, *Rulers of Empire: The French Colonial Service in Africa* (Stanford, 1971); Jesse H. Proctor, Jr., "The Development of the Idea of Federation of the British Caribbean Territories," *Revista de Historia de América*, XXXIX (June 1955), 61–105; Hugh W. Springer, *Reflections on the Failure of the First West Indian Federation*, Harvard University Center for International Affairs, *Occasional Papers in International Affairs*, no. 4 (July 1962); and Arvin W. Murch, "Political Integration as an Alternative to Independence in the French Antilles (unpubl. paper, Yale University, n.d.).

“tutelage.” Since none of the preparatory stages could be applied consciously, consistently, or fully within an informal sphere of influence, it is difficult to say when they ceased to be applied, if at all. While scholars may clearly date significant shifts by the metropole away from segments of an informal empire on the basis of treaties signed, or not signed, protective obligations taken up or dropped, consulate posts opened or closed, these obviously are only the upper rungs of particular ladders and do not reveal the nature of the rungs that preceded. And while some areas embraced within the concept of an informal empire may reflect similarities to formal empire, as when Rui Barbosa in Brazil declared England to have been “the great teacher” of his liberal principles, or Joaquim Nabuco asserted that he was “an English liberal . . . in the Brazilian Parliament,” as though he were “working under the orders of Gladstone”<sup>7</sup>—thereby reflecting an acceptance of the notion of stages in the political transfer of Westminster’s models to unannexed territories—such areas cannot be constitutionally defined. Thus, while many elements inherent in the first manner of decolonizing a formal empire may be present in an informal empire as well, such decolonization is more evolutionary, less clearly marked upon the statute books, and far more open to historical controversy.

If this first manner of decolonization tends, despite caricature, to coincide reasonably well with the image (usually political) held by the defender of empire, the second manner coincides, also despite caricature, with the image (usually economic) held by the enemy of empire. Of course, the first manner may still admit of the idea that decolonization generally occurred when imperial nations found they no longer were obtaining economic benefit from an area (as Eric Williams argues for the West Indies, for example),<sup>8</sup> were unable to commit sufficient resources to an area in the face of concerted internal opposition to their presence (the oft-voiced argument with respect to postwar British India, as another example),<sup>9</sup> or were simply defeated in an outright war by courageous freedom fighters (some could cite Aden) or by the burden of world opinion directed against them once they had crushed such freedom fighters (Kenya comes to mind). That these notions are simplistic makes them no less compelling, for it remains a truism of history that it is what a people believe to be true of the past that is important, not what the good grey historian may say. People are motivated by their beliefs not by the footnotes of scholars. Thus—the New Sentimentalism of Heroic Violence, attributed to Frantz Fanon by those who have not read him carefully, which would see the Boxer Rebellion, the rising in the Bushire highlands, the Mat Salleh Revolt of

<sup>7</sup> Richard Graham, *Britain and the Onset of Modernization in Brazil, 1850–1914* (Cambridge, 1968); Graham, “Sepoys and Imperialists: Techniques of British Power in Nineteenth-Century Brazil,” *Inter-American Economic Affairs*, XXIII (Autumn 1969), 23–37.

<sup>8</sup> Eric Williams, *Capitalism & Slavery* (Chapel Hill, 1944). This argument has been much contested, more recently and effectively by Roger T. Anstey in “Capitalism and Slavery: A Critique,” *Economic History Review*, 2nd ser., XXI (ii/1968), 307–20; Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (Cambridge, 1975); and Howard Temperley, “Eric Williams and Slavery,” in his *British Antislavery, 1833–1870* (Columbia, S.C., 1972), 273–76. See also Edith F. Hurwitz, *Politics and the Public Conscience: Slave Emancipation and the Abolitionist Movement in Britain* (London, 1973).

<sup>9</sup> The view is encapsulated on page 146 of Stanley Wolpert, *India* (Englewood Cliffs, N.J., 1965).

1894–1905, and the recent renewed attack on American periodicals in Canada as efforts at decolonization within still informal empires.<sup>10</sup> That all four of these acts arose from different dynamics does not set aside one generalization: this second scenario of decolonization, whether factually wrong or right, places heavy theoretical emphasis upon economic relations.

But a third process of decolonization may occur in both formal and informal empires. Perhaps it is best (if not adequately) described by Karl Deutsch in his *Nationalism and Social Communication*, in which he suggests six means by which a nation may come to a definition and awareness of itself.<sup>11</sup> One of these is by mutual interdependence, that is, by finding a definition that turns, at least in a major part, upon a symbiotic relationship with another nation. Deutsch cites Argentina in its relationship to Britain, an excellent if traditional example, since even the British referred to Argentina as the Sixth Dominion.<sup>12</sup> The arrested development of Argentina may be compared to that of Canada, because of Britain's economic significance to each, as well as the fear of the United States in each. Indeed, one may argue that both Argentina and Canada showed the tendencies of staple dependency, of the inflexible economies thus induced, and of a reluctance to move rapidly on the road to "decolonization" because of the way staple dependency closed off options and because of the resultant fear of roads not taken and superpowers that might lie at the end of those roads.

Another even more apposite—although much less studied—example of the clear fact that annexation or possession was of little significance to the mutual responsiveness between Britain and another people in the nineteenth century arises from a comparison between Uruguay—not annexed and of the informal empire—and New Zealand, annexed, settled, and turned into the market garden of the empire. To look at the nature of the two economies, whether at the railway networks in terms of mileage or investment, or at the impact of Liebig's Extract of Meat Company in Fray Bentos compared with New Zealand's wool and meat shipments after the introduction of the refrigerated vessel "Dunedin" in 1882, or at any of several other appropriate economic indices to dependence, is to find them similar in all aspects save one: the movement of peoples. Put differently, the only matter of significance that differentiates the degree of dependency of New Zealand and Uruguay upon the United Kingdom, especially after the Mills Committee report of 1862, lies not in the economic sphere, and not greatly in the political, but in the social.<sup>13</sup>

<sup>10</sup> On Fanon, see David Caute, *Fanon* (London, 1970); and Irene L. Gendzier, *Frantz Fanon* (New York, 1973). On the Boxer Rebellion, Victor Purcell, *The Boxer Uprising: A Background Study* (Cambridge, 1963); and on Mat Salleh see K. G. Tregonning, *A History of Modern Sabah (North Borneo, 1881–1963)* (2nd ed., Singapore, 1965).

<sup>11</sup> Karl Deutsch, *Nationalism and Social Communication* (2nd. ed., Cambridge, Mass., 1966). See also William J. Foltz, "Building the Newest Nations: Short-Run Strategies and Long-Run Problems," in Deutsch and Foltz, eds., *Nation-Building* (New York, 1966), 117–31.

<sup>12</sup> See H. S. Ferns, *Britain and Argentina in the Nineteenth Century* (Oxford, 1960), and Arthur P. Whitaker, *Argentina* (Englewood Cliffs, N.J., 1964).

<sup>13</sup> This comparison has not been developed, and it is one I hope to pursue. On Uruguay, see Peter Winn, "British Informal Empire in Uruguay, 1806–1914," unpubl. paper read in 1968 at the American Historical Association meeting in New York City. An expanded version of the paper appears in Winn, *El imperio*

Of course, national identities also were shaped by another form of mutual responsiveness in keeping with the Deutschian model. Deutsch suggests that national identities may arise in part from negative responses to possible absorption from another quarter. We cannot understand Canada's national identity without understanding the role the United States played in the creation of an overlapping informal empire, competing with Britain's more formal one;<sup>14</sup> nor can we understand New Zealand without understanding Australia; Australia without understanding that nation's fear of Japan and later of Indonesia; Uruguay without understanding both Brazil and Argentina, as well as the reinforcing images held by each of the other. In an important sense, then, while there may be many quantifiable indicators of the changing status of informal dependency one people had with another, the real search for an understanding of how the one wished to promote "decolonization" from the other lies in intellectual, rather than in social, political, or economic history.

The idea of the "informal empire" has been with us for some time. In 1934 Charles R. Fay, in his book *Imperial Economy and its Place in the Formation of Economic Doctrine, 1600-1932*,<sup>15</sup> used the phrase to describe Britain's expanding commercial and financial power beyond political jurisdictions. Fay was looking at the impact of the Ottawa Agreements upon British interests already present in the Río de la Plata, however, and some who have taken up the phrase appear to have missed the point that one reference was to a conscious act of choice on the part of the British in an area in which their presence was already felt. Britain could decide, Foreign Office, Treasury, and Admiralty willing, not to annex a territory on the simple ground that, if the economic returns from the area appeared reasonable without exerting political control in a direct manner, why take on the administrative expenses, the risk of entanglement in foreign wars, and the inevitable direct and indirect competition with European rivals that a colony induced? In later years John S. Galbraith would add an important proviso, that a situation of turbulence must not arise within the hinterland of such an area or it might lead to a grave temptation to intervene beyond the frontier to reassure stability.<sup>16</sup> Obviously, informal empires were best exercised over relatively stable societies.

Fay's conception of the informal empire could apply best only to the latter half of the nineteenth century and certainly not well to the earlier times to which it has on occasion been applied. For one must wait until first the consuls, and then the Foreign and Colonial offices, showed a predictive capacity that fell parallel to the needs of the merchants and investors of the

*informal británico en el Uruguay en el siglo XIX* (Montevideo, 1975). On New Zealand, see W. B. Sutch, *The Quest for Security in New Zealand, 1840-1966* (Wellington, 1966). Also important in the context of this point is Geoffrey Blainey, *The Tyranny of Distance* (Melbourne, 1966); H. A. Innis, *The Bias of Communication* (Toronto, 1951); and George Grant, *Technology and Empire: Perspectives on North America* (Toronto, 1969).

<sup>14</sup> Winks, "Canada," in Winks, ed., *The Historiography of the British Empire-Commonwealth* (Durham, 1966).

<sup>15</sup> Charles R. Fay, *Imperial Economy and its Place in the Formation of Economic Doctrine, 1600-1932* (Oxford, 1934).

<sup>16</sup> John S. Galbraith, "The 'Turbulent Frontier' as a Factor in British Expansion," *Comparative Studies in Society and History*, II (January 1960), 150-68.



City of London before the premise Fay posed could, in fact, be fully applied: that a conscious act of choice was made with respect to future economic benefits (benefits not alone to be measured in terms of purely financial gain, of course). To make such an act of choice, the bureaucracy had to have a predictive capability that it could trust; that is, it had to be organized sufficiently well, with sufficient continuity of presumed experts in area-designated offices, and with sufficient liaison between the bureaucratic elements involved to be able to argue a case based upon options which, in themselves, had to be defined upon the basis of predictions. While it could be argued—as Schumpeter, and in a different way, Hobson and Lenin classically did<sup>17</sup>—that post-1870 formal expansion was fundamentally irrational, the creation of an informal empire need not be irrational. If a country entered into a trade agreement for the purchase of x-quantity of goods of y-type, if it supplied the money necessary to help mechanize the production of y-type goods in order to assure the dependability of x-quantity of a-quality across z-years, it must be in a position to exercise at least some control over x and y so that a trading pattern, an enlarged port, a railway, or an enemy made at the price of extraterritoriality could be expected to pay or to offset dividends not just next year but over those predicted z-years. Fay's informal empire depended upon the "rise of the expert," and while we cannot date such a moment more than impressionistically, the founding in Britain in 1857 of the National Association for the Promotion of Social Science will do. Informal empire was a modern phenomenon.

Informal empire was modern in at least three other ways as well.<sup>18</sup> If one may attempt a working definition of imperialism, and suggest that at the least it consists of the events and relationships which emerge as the result of the impact of a higher-technology society upon a lower-technology one, then an important variable in any act of imperialism, formal or informal, is the gap between the high- and the low-technology societies. In general, the historian may argue that the gap was greater in the nineteenth century than at any other time, and the impact was therefore the greater; further, that even though the British did not formally annex certain low-technology areas at first, preferring to use forms of indirect rule, or did not annex some of them at all, preferring the rubric of the protectorate or its modifications, they nonetheless genuinely incorporated such low technology areas into what most of us would recognize was a formal enough imperialism. But where the technological gap was less great, as between Britain and Argentina, informal empire was the chosen solution to the problem of influence, precisely because it was influence and not dominance that was sought.

<sup>17</sup> See D. K. Fieldhouse, comp., *The Theory of Capitalist Imperialism* (London, 1967); and Fieldhouse, *Economics and Empire, 1830-1914* (London, 1973). The second shows a well-argued shift from the position taken in the first.

<sup>18</sup> Here we rule out the frequently silly and usually tendentious comparisons—so popular to British imperialists themselves, especially at the turn of the century—between the Roman Empire and the British, or the use of metaphors of informal empire in relation to the world of Plotinus or the land beyond Hadrian's Wall. One should also note here that I am clearly using a functional definition of informal empire that differs from that used by Ronald Robinson. The crux of the difference rests on the question of choices which either were or, by the flow of events, were not open to the high technology nations.

Informal empire also rested upon the possibility for what may be called ricochet imperialism, itself modern. This term—the conventional Oxford construction is “colonial subimperialism”—is employed here to embrace those examples of informal control (even if later extended into formal control) which involved a colony itself acquiring preeminent power in another area, as Queensland sought over Papua from 1884 to 1906 or South Africa would do in a variety of circumstances; and it also embraces forms of control exercised by former colonies once they have become fully independent, when those forms of control may be shown to have arisen from the former colonial experience, again as with Australia in New Guinea, New Zealand in Western Samoa, or South Africa in Zululand, a Bapu-Tatswana, and Namibia. Informal empire was often based upon the intermediation of a technically independent government with people it in turn treated as colonial. One example would be Portugal, which for a time became a British quasi-dependency, the Portuguese empire showing many characteristics of an empire within an empire.<sup>19</sup>

The third element of modernity arises from big-power conflict. Lenin surmised that some nations would preserve their independence only because of competition between the powers, an early inversion of Deutsch's theme of mutual responsiveness. Such has been the case, although not invariably, in Latin America, where many scholars would argue that Britain did not intervene directly because of fear of the United States. Of course, this is a persistent theme of Canadian history as well,<sup>20</sup> since many Canadians felt that their interests were sold on the block of Anglo-American harmony, whether at the Treaty of Washington in 1871 or at the Alaskan boundary arbitration. Nonetheless, the main point is not blunted: the major powers often found an informal system of control more desirable than formal annexation precisely because of the competition between them; and informal arrangements were attractive in a new age of multiple major powers, a situation which if not politically unique to the nineteenth and early twentieth centuries, was, when coupled with technological capabilities, very nearly so.

At first historians were inclined, when confronted with the apparent contradictions inherent in several formal and informal empires, to take refuge with Walt Whitman who, in *Song of Myself*, said “Do I contradict myself?/ So I contradict myself,/ I am large, I contain multitudes.” But starting in 1953, Ronald Robinson and John Gallagher have maintained that the apparent contradictions of mid- to late-nineteenth-century imperialists were not contradictions at all. They point to the facts that Britain expanded most in the period of 1850 to 1873, the last precisely at the time that Lord Carnarvon and others in the Colonial Office were urging a cessation of expansion; that an empire that did not pay nonetheless continued to grow; and that areas of little

<sup>19</sup> Other papers read at the 1973 AHA meeting examined other areas of informal imperialism. Joseph Tulchin spoke on “A Theoretical Framework for the Study of Informal Empire” and applied it to Argentina, while Joseph J. Malone spoke on “Informal Empire: Case Studies in the Techniques of Control—Arabia (Britain and the Pashaliq of Baghdad).” The commentators were John Cell and Peter Mellini.

<sup>20</sup> A classic example is Donald G. Creighton, *The Story of Canada* (Boston, 1960).

economic value were among the most coveted.<sup>21</sup> Although strongly challenged by Oliver Macdonagh in "The Anti-Imperialism of Free Trade,"<sup>22</sup> and while not without over-simplifications, Robinson and Gallagher have become household words among those whose house is Imperial History, and their view of informal empire is now a familiar one. Grossly oversimplified, their argument is that empire—formal and informal—grew initially upon the basis of free-trade principles (as Fay had strongly implied). Other things being equal, this should mean that the rise of protectionism could lead to a lapse in informal empires—but then, on the principle that the observer changes the phenomena observed, it is also perfectly clear that an informal empire once instituted guaranteed that all things would not again be equal.

In short, Britain was first drawn into informal imperial relationships through a syllogism: It found that it had a technology more sophisticated than any other, having entered the industrial revolution first and, although narrowly, having experienced the most important stages of industrialization more completely than others, thereby temporarily intensifying the technological gap between itself and other societies.<sup>23</sup> Further, this was especially so in cotton, which the world needed in its manufactured form, as did the British middle class, creating both a body of expertise with predictive abilities within Britain in relation to cotton textiles<sup>24</sup> and a demand for overseas markets. As other nations rose to industrial prominence, however, Britain found itself suffering from having gone that route, burdened with antiquated equipment, outmoded industrial practices, and entrenched methods, skills, and techniques. Not being able to meet the new challenge on direct industrial grounds, Britain turned increasingly to the worlds of finance and trade, preferring informal expansion to formal expansion, where possible, a preference it based upon its greater entrepreneurial and capital resources, especially in four areas: railways, loans, banking, and extractive staple-related industries. The

<sup>21</sup> On the "Robinson and Gallagher thesis," see their work, *Africa and the Victorians: The Climax of Imperialism in the Dark Continent* (New York, 1961); "The Imperialism of Free Trade," *Economic History Review*, 2nd ser., VI (i/1953); "The Partition of Africa," *New Cambridge Modern History* (Cambridge, 1962), XI, ch. 22; and Robinson, "Non-European Foundations of European Imperialism: Sketch for a Theory of Collaboration," in Roger Owen and Bob Sutcliffe, eds., *Studies in the Theory of Imperialism* (London, 1972), 117–42. I have drawn heavily on the last essay here. On the criticisms and appraisals of Robinson and Gallagher, see in particular three, Eric Stokes, "Late Nineteenth-Century Colonial Expansion and the Attack on the Theory of Economic Imperialism: A Case of Mistaken Identity?," *The Historical Journal*, XII (ii/1969), 285–92; Stokes, "Imperialism and the Scramble for Africa: The New View," *Historical Association of Rhodesia and Nyasaland*; and the forthcoming book by William Roger Louis, *Imperialism: The Robinson and Gallagher Controversy*, which I have read in typescript. See also Eric Stokes' review essay, "Uneconomic Imperialism," *The Historical Journal*, XVIII (ii/1975), 409–16; and *The Theory of Imperialism and the European Partition of Africa*, Center of African Studies, University of Edinburgh, *Proceedings of a Seminar* (1967). For one application of the "collaborator" model, see Anil Seal, *The Emergence of Indian Nationalism* (Cambridge, 1968). Seal and Gallagher are now at work on their own volume on India.

<sup>22</sup> Oliver Macdonagh, "The Anti-Imperialism of Free Trade," *The Economic History Review*, 2nd. ser., XIV (iii/1962).

<sup>23</sup> See Eric J. Hobsbawm, *Industry and Empire: An Economic History of Britain since 1750* (London, 1968), and P. L. Payne, *British Entrepreneurship in the Nineteenth Century* (London, 1974).

<sup>24</sup> See Peter Harnetty, *Imperialism and Free Trade: Lancashire and India in the Mid-Nineteenth Century* (Vancouver, 1972).

last reinforced the staple dependency of the colonial economies.<sup>25</sup> Later would come the retreat of the 1890s into an entangling formal imperialism.

Thus one need not accept entirely or uncritically the Marxist analysis of why imperialism continued when there were a declining number of colonies to recognize that elements within the Marxist analysis apply especially well to the idea of informal empires and to their elimination, sublimation, or redefinition. For foreign investment on a large scale is of the essence to informal imperialisms, and whether one argues with Hobson that capital export arises from the existence of surplus capital, with the Marx of volume II of *Das Kapital* and of the *Grundrisse*<sup>26</sup> that the explanation lies within the falling rate of profit in an industrial society, or even with Harry Magdoff and the New Fundamentalists that the main cause of capital export rests with the very basic monopolistic structure of industry,<sup>27</sup> the conjunction between the capital element and the human factor must be recognized, and especially so when the time comes to put an end to a relationship. If, as some writers argue, foreign investment came to be financed largely out of retained earnings and locally raised funds, the historian must then study closely the nature of the local society itself and cannot retreat into the comfort of broad economic theory to explain either the rise or the fall, much less the nature and continuity, of informal imperial systems.

It seems apparent, then, that there is as wide a diversity of types of informal empires as of formal, and we must anticipate a diversity of forms of "decolonization" of the informal structures. The comparison between Uruguay and New Zealand suggests one form; the mutual responsiveness of Britain and Argentina suggests a second; the intricate nature of the Anglo-Canadian-American connection suggests a third; the relationship of expanding settler society to indigenous low-technology cultures, as in South Africa,<sup>28</sup> suggests a fourth; the nature of ricochet imperialisms suggests a fifth; the reality of strategic necessities, as seen by the conventional wisdoms of the time and under the impact of the prevailing military hardware—although not discussed here since one may ultimately subsume most strategic concerns under economic ones—suggests a sixth (Corfu, Iceland today, a corner of Abyssinia under the Treaty of Ucciali, and Crete are examples).<sup>29</sup> Nor do these variants include the less finite but no less important forms of relationships, those created by the dominance of a language or a literature, or by the declaration

<sup>25</sup> On dependency theory, see in particular James D. Cockcroft, André Gunder Frank, and Dale L. Johnson, *Dependence and Underdevelopment: Latin America's Political Economy* (New York, 1972), and Robert I. Rhodes, ed., *Imperialism and Underdevelopment: A Reader* (New York, 1970).

<sup>26</sup> David McLellan, ed. and trans., *Der Grundrisse* (New York, 1971). The question of dependency, alienation of land, and the development of "modern" market economies based on pre- or proto-capitalist indigenous societies is complex wherever it arises. For one major work in the area, see A. G. Hopkins, *An Economic History of West Africa* (New York, 1973).

<sup>27</sup> Harry Magdoff, *The Age of Imperialism* (New York, 1969). See also Arghiri Emmanuel, *Unequal Exchange: A Study of the Imperialism of Trade* (New York, 1972).

<sup>28</sup> See chapters 7 through 9 of Monica Wilson and Leonard Thompson, eds., *The Oxford History of South Africa*, I, *South Africa to 1870* (Oxford, 1969).

<sup>29</sup> Raymond Matton, *Corfu* (Athens, 1960); A. H. M. Jones and Elizabeth Monroe, *A History of Ethiopia* (Oxford, 1960); W. F. Monk, *Britain in the Western Mediterranean* (London, 1953).

of responsibility for an entire people regardless of where they go and irrespective of the annexation of the lands from whence they come (which in effect occurred with the establishment of the Western Pacific High Commission in Suva in 1875, for example).<sup>30</sup>

Despite the presumed element of predictability in informal empires and the assumption of returns at least potentially more open to guarantees in practice, the informal empires did not prove to be any more tractable, predictable, or rewarding than the formal ones. D. C. M. Platt and Peter Smith<sup>31</sup> have both argued that precisely because the British presence did not have the putative certainty of permanence, or even the illusion of permanence as in India, informal imperial controls were surprisingly weak. Platt asserts—though from an utterly different perspective, to the same logical conclusion as those who seek to refute the New Fundamentalists—that to understand informal empire one must understand the individual investor, financier, trader, contractor, manager, and concessionaire;<sup>32</sup> in short, that economic theories of imperialism, and especially of the informal variety, have not given sufficient attention to private individuals *as* individuals rather than as Economic Men. While monopolists tended to behave in much the same way whether at home or abroad, so that “empire” becomes an umbrella word without real meaning, the railway companies operating from Sheffield behaved in the same way as the British railway monopolies in Latin America. Where a fixed, or severely limited, market existed for a product, an organization developed to exploit opportunities to restrict its production, whether in an informal empire or at home, whether on the basis of expatriate capital or domestic, whether based upon indigenous control of the betel-nut trade or on Hausa, Fulani, or Ngati Poneke leadership. The examples thus far best researched—of the foreign-dominated nitrate combinations of Chile, the international iodine syndicate, and the Borate syndicate, contrasted with the native-dominated organizations for valorization of Brazilian coffee, the promoters of cocoa production in São Thomé or Ghana, or the wine monopolists of Argentina—could, with less detail, be repeated around the globe. The Latin American case suggests that the example of cotton may be generalized upon: it was the nature of the trade or industry which basically determined its need to control a market, whether under foreign or native direction.

<sup>30</sup> Deryck Scarr, *Fragments of Empire: A History of the Western Pacific High Commission, 1877–1914* (Canberra, 1968).

<sup>31</sup> D. C. M. Platt, “British Diplomacy in Latin America Since the Emancipation,” *Inter-American Economic Affairs*, XXI (Winter 1967), 21–41; Platt, *Finance, Trade and Politics in British Foreign Policy, 1815–1914* (Cambridge, 1968); Platt, *The Cinderella Service: British Consuls since 1825* (London, 1971); and Platt, “Economic Imperialism and the Businessman: Britain and Latin America before 1914,” in Owen and Sutcliffe, eds., *Studies in the Theory of Imperialism*, pp. 295–311. As Graham points out elsewhere, while Platt sees himself in opposition to Robinson and Gallagher, some of his evidence can be read in their support. See also Benjamin J. Cohen, *The Question of Imperialism: The Political Economy of Dominance and Dependence* (New York, 1973).

<sup>32</sup> Some prefer to refer to *compradors* rather than collaborators, especially in an Asian context. See Norman G. Owen, *Comprador Colonialism: Studies on the Philippines under American Rule*, Michigan Papers on South and Southeast Asia, no. 3 (Ann Arbor, 1971); and Yen-p'ing Hao, *The Comprador in Nineteenth-Century China: Bridge between East and West* (Cambridge, Mass., 1970).



No less important were the variables which helped to determine the manner in which a people might disengage themselves from another. (These variables apply equally well, indeed quite possibly better, to the comparative study of the differing nature of white settler-native peoples relations in Canada, Australia, New Zealand, and southern Africa.)<sup>33</sup> The first variable is the nature of the indigenous society, in all of the multiplicity of variants societies may show. For we must take care not to be unconscious racists, or to embrace simple-minded assumptions that low-technology people are all alike. To accept a single explanation for the rise of collaborators within indigenous societies is to accept the harmful implications lurking behind the anthropology of the Lucien Lévy-Bruhl of *How Natives Think*.<sup>34</sup> Obviously each local society creates a collaborator class out of its own culture, and the nature of the collaboration that follows will be based upon quite different forms of imitation, role playing, and (to use Deutsch's phrase) interlocking ranges of transaction.<sup>35</sup>

The second variable is the nature of the imperial power and its people, for each high-technology nation perceives of the role of technology differently, places different values upon specific technologies, and conditions those who go overseas in the service of each technology differentially. Within a managerial group the nature of interaction with the local culture will vary depending upon such matters as the generation, training, marital status, income, rank, expectations, and goals of those sent out. (Clearly here is one instance where the Cliometricians can come to our aid.)

The third variable is the relationship between the nature of the terrain in which the meeting of cultures takes place, the value of the land in relation to the needs of the two cultures, and the nature of the resources in terms of world demand at specific times.

And the fourth variable is the degree of commitment the imperial power wishes to make to the retention of a territory, a treaty right, a most-favored-nation clause, a railway contract, or a mining lease. Commitments change dependent upon unpredictable urgencies, as when the British sought to control Akaroa (in New Zealand) in the face of mistaken expectations about French intentions, or upon predictable contingencies, as when the British scuttled the Anglo-Japanese alliance in the face of Canadian pressure and American concern. Clearly Britain was often unwilling to commit her strength unreservedly to informal empire.

Having examined this one variable, for example, Platt concludes that limitations on the discretion and authority of foreign entrepreneurs were great, and that because of fear of the United States Britain intervened very seldom in Latin America. His judgment, not yet sustained in all particulars, helps explain the process of decolonization, for as he writes, the "only real safeguard" for the foreign entrepreneur was his "indispensability, and when

<sup>33</sup> The following argument is touched upon in Winks, *The Myth of the American Frontier: Its Relevance to America, Canada and Australia* (Leicester, 1971), and I hope to develop the argument further in an extended essay.

<sup>34</sup> Lucien Lévy-Bruhl, *How Natives Think* (London, 1926). First published in French in Paris in 1910.

<sup>35</sup> Karl Deutsch, *Political Community at the International Level* (Garden City, N.Y., 1954).



local competitors and substitutes developed he was soon displaced.”<sup>36</sup> Because there are powerful and effective informal empires today, because thinking Americans are aware of massive pressures by their government and their businessmen overseas, perhaps many apply to the years prior to World War I conditions truly valid only for the more recent period. While any industrial government today presumably assumes a responsibility to promote financial and commercial interests overseas, until the D’Abernon Agreement with Argentina in 1929 the British government insisted that its responsibility was limited to supplying commercial information. Nor in the nineteenth century did Britain seek the vital materials wanted so desperately by industrial powers today, for she had her own fuel, which was coal. Further, Britain was in no position, given the realities of the world, to achieve hegemony, or invariably to refuse disengagement when it was demanded of her.

As we know, where a formal unit of her formal empire was involved, certain assumptions tended to guide British actions. As suggested, Britons sought to export a Westminster model of Parliament and to establish an independent judiciary, a neutral civil service, and (often) the local equivalent of Sandhurst. They believed that “national unity” must be encouraged before decolonization could proceed.<sup>37</sup> They reasoned that where small colonies lacked the potential for a “viable economy,” they might be tidied up by being pressed into some form of convenient (to the British) federation. And they held that institutions were the best indices of progress. Lacking the power, desire, or purpose to apply measures to achieve these goals within an informal situation, recognizable measures were not easily applied, so that the role of the collaborator groups was more difficult, tenuous, and exploitative than in annexed territories simply because such groups were not to be used as the agents (or tools, if one prefers) of a set of specifically mandated changes.<sup>38</sup>

Resistance to informal empire was therefore of a different nature than within the areas of the world map painted red. To be sure, the same typology could be observed, of primary resistance, secondary resistance (as the phrases have it), of quasi-millenarian movements, of hostile welfare associations, trade unions, and churchly bodies, and of sophisticated political opposition. Within informal empires, such forms of resistance could never be clear cut. As generations competed for elite leadership—a competition that often turned upon ever larger access to education by each succeeding generation, either within schools and training schemes established by the informal imperial power, or within traditional forms that clearly created no adequate means of dealing with the challenges brought by the interlopers’ new technology—those who wished to oppose had first to cooperate. The manipulation of prices,

<sup>36</sup> Platt, in Owen and Sutcliffe, eds., *Theory of Imperialism*, pp. 295–311. On Chile, see Harold Blakemore, *British Nitrates and Chilean Politics, 1886–1896: Balmaceda and North* (London, 1974).

<sup>37</sup> Bernard Semmel, *Imperialism and Social Reform* (Cambridge, Mass., 1960).

<sup>38</sup> The British helped create their own Opposition in Sierra Leone precisely because that colony was not producing an effective Opposition, contrary to the needs of colonial theory. But the British did not need to create oppositions in Central America or Greece or Argentina. Their battles were fought for them in such places.

the transformation of agricultural practices, the creation of marketing agreements, and interference with systems of land tenure, all of which could occur in informal as well as formal situations, drew those who formerly were outside the influence of the local bureaucracy into its circle. One result was that nationalist or antiforeign movements could run downwards through a series of competing elite groups, each able to command a wider circle of support, and at the same time could also be the product of a populist ground swell, catching the middleman truly in the middle. In a formal empire, any great power could take steps to rectify such a situation in its favor; where an imperial power was dealing with a putatively independent country, it could do little save ride out the storm, often unsuccessfully.<sup>39</sup>

Thus the role of the collaborator (or *comprador*) was critically important within the informal empire. (Collaborator here is meant to have none of the pejorative meaning given to it by its association with *quisling* in World War II, for those who employ the term intend it as an unemotive, descriptive word for a sociological phenomenon.) Where informal empires arose, the relationship between the two elements was initially one stemming from conditions of relative equality. The local leaders, whether of business or politics, wished to have the benefits—as they defined them—of technology and trade. Having grasped the end of the stick, they sought to maintain their monopoly over communication with the power that could provide these benefits. As the European power explored more carefully the benefits that might flow to it from working through such collaborators, it usually found that the initial group was inefficient or otherwise not the most appropriate, leading to an uneconomic relationship which could be little improved upon.

In any event, British businessmen were well aware that to have no options was to have no future, and that options were eliminated in direct proportion to the degree to which they depended upon a single collaborating group. The imperial nation logically enough wished to exert increased authority over the initial collaborating element while trying to create new elements as well; this could be done only with the greatest delicacy, however, for the very stability of the society could rest on the continued preeminence of the initial collaborative group, and above all the businessman wanted stability. His choice often was one of inaction then. Still, the first group of collaborating merchants would protest if they found their interests declining—this happened too in formal colonies, of course, for what else was the Montreal annexation manifesto of 1849?—and would fear that the British would be only too happy to shift their trade, contracts, and loans to a younger, better educated, more technologically alert group of collaborators waiting in the wings. Sometimes the fear was realized. Having seen one group shunted aside, other elites might

<sup>39</sup> Eric Stokes, "Traditional Resistance Movements and Afro-Asian Nationalism: Towards a General Theory," unpubl. paper presented at the University of London's Institute of Commonwealth Studies Postgraduate Seminar on October 29, 1970; and Stokes, "Traditional Resistance Movements and Afro-Asian Nationalism: The Context of the 1857 Mutiny Rebellion in India," *Past and Present*, no. 48 (August 1970): 100–18.

collaborate beyond the point of no return rather than take the risk of protesting too much, since they almost always had no support from below that could be counted upon, the populist form of protest to an imperial presence being based upon the tenets of class warfare. As Eric Stokes has noted of, among other areas, unannexed but “protected” parts of the Indian Empire, “the processes of modernity will set up a constant tendency to displace traditional by more modern elites.”<sup>40</sup> Thus arose the recurrent crises of displacement.

In the long run, however, the British generally were not successful in informal situations—as they often were successful in formal ones—in keeping a single group from taking control of the channels of communication. For in a formal colony, those channels were kept open for a great diversity of reasons, while in an informal relationship the channels were maintained largely, although not exclusively, for reasons of trade and finance. This being so, and trade and finance being subtle and shifting matters from which one could not divert young turks by sending them off after some constitutional bauble, one had to stick with the collaborator groups one had, cutting the options down severely and reinforcing a tendency toward inaction. While the presence of an informal empire could draw the potentially dissatisfied, especially those unable to live from landed incomes, into increasingly sophisticated forms of government employment, the imperial nation had no control over the middle and lower echelons of bureaucracy within the local society. Petty bureaucrats in India had reason to be loyal to the system just as poor whites in the pre-Civil War American South were the staunchest supporters of slavery, although they owned no slaves themselves: they did not want anyone to kick that ladder out of place by which they defined their present status and future goals. But petty bureaucrats in Latin America or Eastern Asia had no reason to be loyal to the British, and it was from their ranks that opposition to the entrenched collaborators at the top came. They were likely to feel more quickly than others the effects of dislocations within the world’s market structure, and it seems clear that the price inflation of the decade prior to World War I helped focus discontent at the mid-level against the older forms of informal empire. It was then that British engineers in Thailand at work on the railroad, in Constantinople at work on the sewers, and in Argentina at work on the meat-packing plants began to realize that they soon would no longer be needed or wanted, for local counterparts would take over.

Which brings us squarely to that collaborator around whom we have been skirting, and back to the brilliant insights of Ronald Robinson.<sup>41</sup> An unusual aspect of Robinson’s work, perhaps because of the ambivalent nature of imperial contacts with Egypt and the Sudan—the areas to which he and his collaborator John Gallagher first applied themselves—is that his model applies no less well to informal than to formal situations. To be sure, detailed applications by his model have, to date, been limited to preindustrial societies, and certain adjustments are called for within the model. Nonetheless,

<sup>40</sup> *Ibid.*, 103.

<sup>41</sup> See note 21.

working along somewhat parallel lines, as we have seen, several scholars have reached similar conclusions for Latin America and the Far East, without specific reference to or perhaps even full awareness of Robinson's main points.<sup>42</sup> All have a basic point in common, however: to understand imperialism, whether expressed in formal or informal modes, one must understand the mediating groups in the society being encroached upon, in order to examine how such groups interweave their needs with those of the expansive power, until—as particularly in the case of informal empire—a type of equilibrium between those needs temporarily is achieved. It follows that if we are to understand decolonization in this context, we must put two questions: how does the woven fabric fray? and may the fraying—or, to abandon the metaphor, the *perception* of decolonization—not simply be a consequence of the fact that an equilibrium had never been achieved, the mediating groups having failed rather than succeeded? For either question one must study individuals and groups within the local society, treating them not as economic integers in some broad theory but rather with all the proliferating variety common to a Burkean view of life.

As Robinson argues, it is clear that where the local society is based upon a large subsistence sector, and has only a small export-import sector, real or potential, to its economy, informal empire is not a rational arrangement by which an equilibrium of needs is achieved, and the imperial power may be tempted to intervene directly in order to exercise sufficient power to reshape the balance between the sectors. But intervention need not mean annexation, as it did not in the Argentine or in Uruguay. In both cases the problem for the British was that—partially because of inadequate political unification (for which the British devoutly wished in Central America)—no one group in either nation could expand the import-export sector of the economy rapidly enough to meet Britain's needs. (So much for the easy notion of divide and rule.) Later, however, as both nations moved to greater unification, stability, and sophistication, the *porteños* were able to overbalance the backlands. As a local economy diversifies into local capital formation, its commercial dependence upon the imperial power lessens, unless that capital is channelled into noncompetitive areas or becomes subsidiary to that of the imperial power; thus while informal empire may be built upon simple economic needs, it endures only through the maintenance of a delicate balance of forces on both sides, a balance the imperial power will be tempted to manipulate through the local political system.<sup>43</sup> This may produce what another generation will call Uncle Toms, or what some will see as the "bought politician"—who will seem inimicable to his own nation's deeper needs as defined by the rising young turks. For the young turks will always rise, and knowing this, the

<sup>42</sup> See also the work of Frank J. Moreno, John H. Rowe, and, most particularly, André Gunder Frank, *Capitalism and Underdevelopment in Latin America: Historical Studies of Chile and Brazil* (rev. and enl. ed., New York, 1969).

<sup>43</sup> I am indebted to four graduate students at Yale, Gilbert Joseph, David Holm, Florencia Mallon, and Steven Rosenthal, for conversations on Mexico, Thailand, Chile, and Turkey respectively.

imperial power will stand ready to disengage when the game is no longer worth the candle, since matters of national prestige do not, and matters of preemptive annexation seldom need arise.<sup>44</sup>

Put differently, the equilibrium fails when the imperial partner seeks too large a share of profits for the internal partner to justify to those it must keep in domestic equilibrium to itself, or if the imperial partner demands publicly visible, and therefore politically unacceptable, forms of gratification. When this occurs, the terms of collaboration are no longer mutually acceptable, for one side has transgressed against the need of the other to be seen capable of acting independently. This being so, the internal partner can no longer hope to maintain political control at home and one of two things will happen: it will cease collaboration in order to maintain that control in the face of the young turks, or it will fail to cease in time and be overthrown by a different group. Either way, the imperial power must adjust its role, and either way the equivalent of a form of decolonization will occur, even if only temporarily. Since much of this change will not be visible to the casual observer, however, will not be demarked by flag lowerings and raisings, will not involve the dismissal of a lieutenant-governor or, perhaps, even the abrogation of a trade treaty, disengagement will be as subtle and difficult to analyse as was the original acquisition of power.

One additional condition remains to be taken into consideration: the prevailing wisdom on the world balance of power. By this we do not mean the actual balance of power, obviously, but rather what strategists had to say about it in relation to perceived strategic needs, predominant weaponry and technology, and countervailing tensions between other powers. Hence the importance, for example, of a Halford John Mackinder.<sup>45</sup> (We have already seen the necessity of understanding Britain's disengagement from Japan in relation to Canada and the United States, and we have noted D.C.M. Platt's strictures on the image of an all-powerful and insidiously pervasive British presence in Latin America.) To be sure, the British worked to abolish or break into existing monopolies, to effect by persuasion, compromise, and trade a lowering of tariff barriers, and after the 1860s to modernize the fiscal, legal, and political systems of societies which wished British loans.<sup>46</sup> They also wished to use these "modernized" systems to assure that the collaborators involved in the export-import sector of the economy, which by definition was regarded as the more modern or progressive, would exercise political control. Yet what they wished was seldom achieved for long, and Britain sought disengagement.<sup>47</sup>

<sup>44</sup> One hopes that the work of Ronald Wraith and Edgar Simpkins, in *Corruption in Developing Countries* (New York, 1963), and Onofre D. Corpuz, *The Philippines* (Englewood Cliffs, N.J., 1963), has led some scholars to be less glib about the notion of corruption as measured by Gladstonian liberal principles.

<sup>45</sup> See especially Halford John Mackinder, *Democratic Ideals and Reality: A Study in the Politics of Reconstruction* (London, 1919).

<sup>46</sup> For early examples of this view see William Woodruff, *Impact of Western Man: A Study of Europe's Role in the World Economy, 1750-1960* (New York, 1967); and I. Robert Sinai, *The Challenge of Modernization: The West's Impact on the Non-Western World* (New York, 1964).

<sup>47</sup> See D. A. Low, *Lion Rampant: Essays in the Study of British Imperialism* (London, 1973).

To disengage from a formal imperial responsibility would prove difficult, though not invariably so, for since there were specific powers to be handed back, specific stages to be passed, and a clear, if perhaps too misleadingly political, final goal in mind, the imperial power knew where it stood. When the ties were informal, left to the realities of the world market, or to the whims of democratic Parliaments which might or might not choose, for any of a number of reasons, to staunch the flow of immigrants, or skilled artisans, or capital, or rails to another land, under the demands of free trade or fair trade or moral boycott or some other compulsion of the moment, then those ties were far more difficult to disentangle. It is a Mexican proverb, oddly enough, that says that to divorce one's wife is simple; to divorce one's mistress impossible. Perhaps the final irony of informal empire was that precisely because it was informal, it was the most tenacious of all. For what else could follow in the wake of withdrawal under such circumstances, there being no new flag of independence to run up the pole, what else except internal struggle between shifting, bereft collaborators seeking new positions on the rungs of an endless ladder?



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## Reviews of Books

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### GENERAL

JACQUES BARZUN. *Clio and the Doctors: Psycho-history, Quanto-history, & History*. Chicago: University of Chicago Press. 1974. Pp. xi, 173. \$7.95.

For more than twenty centuries, historians have been laboring to satisfy the human craving for a comprehensive and intelligibly written past. Their task has not always been easy, for man is distressingly complex, if not perverse, events and forces conflict, plans go awry with unintended results, and records have a habit of being either appallingly sparse or overwhelmingly abundant. Far too often, historical events are the result of conflicting forces which defy repetition or measured ranking, and their impact is subject to continuous debate. As a result, historians have become "scientific" only to the extent that they subordinate their prejudices. Their much-touted "method" consists of little more than thorough research, informed judgment, and a common-sense weighing of the facts. Yet, it is precisely this indeterminacy which gives history its enduring charm, for its purpose is to cultivate the mind, not to convey facts.

Recently, this traditional view of the historian's craft has fallen under heavy attack from dissenters bent on converting history into a behavioral science. The assault is not unpredictable in an age plagued by problems which demand immediate solution, and for that matter, it is not really new. The appeal to psychology and other disciplines dates from Homer and Plutarch, and the modern effort to lure history into the Procrustean bed of analytical models finds its roots in phrenological determinism. Barzun's objection to these "doctors," the psychologists, sociologists, and quantomaniacs who are "attempting to rescue Clio from pitiable maidenhood by artificial insemination," is quite simply that they are anti-historical and represent pseudosciences that confuse vocabulary with method. History is, after all, the study of change produced by the interaction of people and forces over time, and the study of a single factor at the expense of all others distorts more than it enlightens. It is all very well to argue

that Cleopatra was fondling a phallic symbol when she clasped an asp to her bosom, but it tells us nothing and reduces a great historical tragedy to a tasteless act of perversion. Even this might be acceptable if the behaviorists provided a unified theory instead of a sea of conflicting interpretations, but how are we to choose between the findings of an intrepid conclave of the AHA which traced German fascism to Hitler's undescended left testicle, and a recent study which explains the same phenomenon as the by-product of an encephalitis epidemic. The answer, of course, is that one, and probably both, will be consigned to well-deserved oblivion. The most telling indictment, however, is that behavioral models assign man the role of helpless automaton in the grip of overwhelming forces which dictate his past, present, and future, a condition which reduces human will and history to a null entity.

Barzun's defense of his craft is the charming, witty, and perceptive work of a mature and urbane scholar. It says what needs to be said at a critical stage in the development of historical thought and method. While it will undoubtedly draw heavy fire from the advocates of the "New History," it will stand as a classic of its kind when their work is being cited as hackneyed and dated.

ROBERT J. PARKS  
*University of Wyoming*

ERIC VOEGELIN. *Order and History*. Volume 4, *The Ecumenic Age*. Baton Rouge: Louisiana State University Press. 1975. Pp. xvii, 340. \$15.00.

The appearance, eighteen years after volumes two and three, of the fourth volume of Eric Voegelin's *Order and History* is noteworthy in itself. But the fact that in the interval the originally projected pattern of the work has been dramatically altered (or "broken," as the author puts it) heightens the significance of the event. The result is a theoretically more coherent, if much more complex, philosophy of history than Voegelin offered in the earlier books. *The Ecumenic Age* is both a singularly important and exceptionally difficult book.

In the previous volume Voegelin adumbrated a

plan for treating the symbolization of order in concrete societies as a linear development through time, but always with a clear recognition that history is open-ended. His later studies of the empires of the Ancient Near East and other civilizations led him to conclude that the differentiation of the compact cosmological interpretations of man's existence in history through revelation in Israel, philosophy in Ancient Greece, and the Christian experience of salvation are not unique in their mythopoetic expressions of "man's participation in the flux of divine presence that has eschatological direction" (p. 6).

The very organization of the fourth volume reinforces Voegelin's shifting perspective, for it consists of a series of special studies which range over parallels in history without respect to sequence. The constancy of man's experience and the symbolic means used for interpreting that experience engage Voegelin's imagination and bind the disparate studies into a unified theory. The common experience out of which the symbolic interpretations emerge is the consciousness of living in a state of "*Metaxy*" (Plato's term), which is the tension between the finiteness of the world and the intimations of divine presence partially open to man.

The chapter from which the title of this volume is taken concentrates on the period from the rise of the Persian Empire to the fall of Rome—the "axis-time" during which the rise of the great religions of the world (simultaneous spiritual "outbursts") coincided with the development of multi-ethnic geographic empires which drew on the religions for their claims to universality in both the physical and historical senses. The other special studies in the fourth volume, including the origins of historical speculation, St. Paul's understanding of the resurrection, the Chinese *ecumene*, the concept of universal humanity, etc., all provide empirical evidence to enlarge our understanding of the central thesis of man's quest for the ground of being and the relation of that quest to the problem of order in the history of pragmatic societies.

*The Ecumenic Age* confirms the view of those who have long held that Voegelin is the only contemporary philosopher of history whose mastery of historical sources equals that of the late Arnold Toynbee, and whose philosophical acumen by far exceeds Toynbee's.

WILLIAM C. HAVARD  
*Virginia Polytechnic Institute  
and State University*

M. I. FINLEY: *Democracy Ancient and Modern*. (Mason Welch Gross Lectureship Series.) New Brunswick: Rutgers University Press. 1973. Pp. x, 118. \$6.00.

This is the text of a series of three lectures delivered in New Brunswick, New Jersey in 1972. It has the advantages and disadvantages of its origin: unrelated organization and different kinds of presentations, in which emphasis is not placed upon the careful and systematic sifting of evidence in an effort to reach conclusions but upon simple statements of belief, pleasantly spaced by copious quotations. Nevertheless, there is much here to think about. Shifting back and forth between the ancient and the modern world, the author develops a number of themes. Of these the most interesting is the apparent paradox between political leadership and participatory democracy in Athens. Although this condition would normally be expected to lead to a confrontation ending in stagnation or anarchy, the author shows us how, on the contrary, peaceful coexistence prevailed for a long time. He does not really face up, however, to the problem that Greek democracies failed to produce, for good or ill, a Greek nation or a united country. He spends considerable time defending the Peloponnesian War, at least in the sense that it was undertaken with the consent of the Athenian people and that it benefitted the poor rather than the rich. In these latter points he fails to get the question of class struggle onto center stage, although this reviewer senses it waiting in the wings. Its entry would have been interesting, for no one has ever proved the existence of class struggle in fifth-century Athens. In the third and final section on "Socrates and After" there is a more unified and better-organized treatment of a very important aspect of Greek life, freedom of speech and civil liberty. All in all, the author apparently favors a return of democracy to something like the Athenian model, at least in its spirit and its approach to government.

ROBERT K. SHERK  
*State University of New York,  
Buffalo*

DAVID KNIGHT. *Sources for the History of Science, 1660–1914*. (The Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1975. Pp. 223. \$11.00.

Here is a useful and readable introduction to materials for research in the history of science: journals, books, manuscripts, and physical objects. The emphasis is on chemistry, physics, and natural history in nineteenth-century Britain, but students of other periods, countries, and disciplines will find valuable information. The numerous examples of pitfalls might have been supplemented by more specific examples of how to avoid them. Moreover, the index sets a bad example by not covering the footnotes; this is not a "handbook" in

which one can quickly look up detailed information. But it can be recommended to graduate students and others who want to know how historical knowledge about science is gleaned.

The paperback edition is unfortunately available only in the United Kingdom.

STEPHEN G. BRUSH  
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College Park

PHILIP S. BAGWELL. *The Transport Revolution from 1770*. New York: Barnes and Noble. 1974. Pp. 460. \$17.50.

Comparisons with T. C. Barker and C. I. Savage, "An Economic History of Transport in Britain" (third revised edition, 1974) and H. J. Dyos and D. H. Aldcroft, "British Transport" (1969) are both inevitable and necessary. All three works are comprehensive narratives of transport developments within Britain from the eighteenth to the mid-twentieth century, although Dyos and Aldcroft terminate their study at the commencement of the Second World War. Bagwell's analysis is the most expansive of the three in that a review of coastal shipping is added to thoughtful, detailed, and lively commentaries upon the relative roles of inland waterways, railways, and road transport, with internal air transport being briefly touched upon in the final chapters.

Developments subsequent to the Second World War are fully discussed with particular emphasis upon the influence of transportation policies of the various governments. The author provides a pithy appraisal of a somewhat restricted bibliography, but, more important, provides extensive footnotes to each chapter, with ample reference to numerous primary sources, particularly government papers. Throughout the book, much use is made of statistical material, presented clearly by a plenitude of tables, with references to the less familiar twentieth century rather than the well-worked nineteenth century.

By providing such a comprehensive review of the existing research, Bagwell is at once the master and prisoner of his sources. The weaknesses inherent in his study are those of the discipline itself—it is clear that there are still areas where research must be undertaken. Some facets are not considered in the same degree of detail as are others, although it might be argued that they are of equal significance. The author has done transport history a great service, however, by pinpointing, albeit tacitly, those areas where future research should occur. His eminently readable style and competent handling of a necessarily factual account have provided transport historians with a

particularly valuable addition to this flourishing branch of economic history.

JAMES R. HEPPLER  
University of Hull

GEORGE ROSEN. *From Medical Police to Social Medicine: Essays on the History of Health Care*. New York: Science History Publications. 1974. Pp. 327.

This book brings together a group of thirteen essays on the relations of medicine and society published in eight different journals between 1944 and 1973. Two general themes run through the collection. The first is that in any society social, economic, and political conditions and theories influence or even determine the type and prevalence of disease as well as the society's policies, plans, and proposals for health care. Essays examine this theme historically with examples of mental illness and pellagra, and they relate it to the development of proposals for medical care and public health programs during times of different views of political economy. Examples come from Germany, France, England, and the United States. The second theme is that historical examination and analysis are necessary for understanding modern institutions of medicine and the way medical and health care is provided. It supports Rosen's idea that medicine is a social as well as a biological science. As a whole, the essays provide an excellent study of many aspects of health care.

The essays were originally addressed primarily to audiences of health professionals, who presumably are less committed than historians by profession, temperament, or interest to the value of history and less inclined to think in social terms. For them, the thematic view may be more original and significant. For social historians, the essays will provide much good meat and sound historical scholarship on the relations of medicine and society from one whose medical knowledge provides a background that many social historians obviously lack.

Inevitably, there is a certain amount of duplication among the essays, making readers hope that Rosen will in time prepare the comprehensive history of medical care for which he is eminently qualified as physician, sociologist, public health expert, and historian. Meanwhile, it is useful to have these related essays in convenient form.

JOHN B. BLAKE  
National Library of Medicine

THOMAS F. GLICK, editor. *The Comparative Reception of Darwinism*. (The Dan Danciger Publication Series.) Austin: University of Texas Press. 1974. Pp. ix, 505. \$15.00.

A collection of seventeen essays by fifteen authors cannot be expected to maintain a standard level of treatment, even when they are the result of a conference sponsored by the American Council of Learned Societies and the University of Texas. With no uniformity of procedure, with no average level of coverage, and without even a standardized definition of what Darwinism is, the book is of limited use to the general historian who wants one worthwhile work on the subject—or even one complete treatment of the country of his choice. The general historian, for example, might turn to M. J. S. Hodge's bibliographical essay on England, which mentions seven publications as, respectively, "unmatched," "remarkable," "masterly," "exemplary," "lucid and penetrating," "incisive," and "important." Hodge mentions eight others with less enthusiasm: "careful analysis," "detailed discussion," "still useful," "also valuable," "enlarge our knowledge" (two articles), "some detail," and "standard." He discusses others at more length; but how is the general historian to guess that I, for example, would argue seriously with most of the publications on the first list and would rely with confidence on most of those on the second list? I am not saying that I am right and Hodge is wrong, but this is a field full of snares for the uninitiated.

The volume is idiosyncratic, however, in the best sense. It is a fascinating display of the various procedures that are used in "reception of" studies and of the puzzlements caused when a major scientific development becomes involved with older, perhaps more powerful, and sometimes antagonistic movements of belief existing in a society—such as evolution, progress, social amelioration. It is easy to condemn modern authors for discussing these other things instead of the specific reactions to Darwin's science, but what is one to do if these things *were* what was discussed most at the time? My feeling is that this book is well suited as the "text" for an advanced seminar on method. By text I mean that the students would tear some of the essays to shreds and press hard even on the most substantial ones. There is not yet a satisfactory methodology for handling this kind of subject. This book could be the stimulus—or irritant—for developing one.

Under these circumstances, a reviewer can only indicate what he likes and not complain too much about what is not done. My choice for first place is William Montgomery's essay on Germany. His treatment of specific scientific reasons why some biologists opposed Darwin is the kind of work the field needs. I admire Robert Stebbins' demonstration of why Frenchmen were content with their own *transformisme* and unable to see that Darwin was either new or important. Frederick Burk-

hardt's account of papers published by the major learned societies of England and Scotland is solid and satisfying, a job that needed to be done. Hodge discusses the scientific-intellectual scene in England fairly widely, while warning that such a description does not explain in detail the reception of Darwinism.

Darwinism in the United States is too big and too confused a subject to be covered in one paper, although Edward Pfeifer tries. His account of the development of neo-Lamarckism is well done. Michele Aldrich, in her bibliographical essay on Darwinism in the United States, summarizes valiantly, draws attention to "carelessness in the American literature about whether a response was triggered by a writing of Darwin . . . or by another thinker" (p. 207), and concludes—if I interpret her correctly—that it is time for someone to clean up the mess.

Russia seems to me to suffer, even with two essays, Alexander Vucinich's "Biological Sciences" and James Allen Rogers' "Social Sciences." Both authors in my opinion have done their best writing elsewhere; for example, Rogers' article in *Isis* in 1974 seems to me simply more interesting than the Russian articles in this volume. The same is true for David Hull in "Darwinism and Historiography"; he shows his subject to better advantage in the introduction to his own anthology, *Darwin and His Critics* (1973).

Ilse Bulhof, on the Netherlands, has a specific thesis. "From a sociological point of view, Darwinism was received by and on behalf of the liberal bourgeoisie" (p. 289). Plausible, but is it demonstrated? How should it be demonstrated? Najm Bezirgan, in a short essay on the Arab countries, is sure that it was "the philosophy of evolution and progress" that was being introduced; but, compared to other Western ideas, such as those of Comte, Mill, and Spencer, Darwin's own ideas were not very important. Harry Paul has an international essay, "Varieties of Catholic Reaction"; it is only in a few of his excellent pages that Italy is discussed in the volume.

Anthony Leeds argues at length that virtually all forms of nineteenth-century evolutionism that affected or still affect the social sciences were non-Darwinian, indeed, were anti-Darwin. He quite approves of this: "The ordering of life characteristic of the human species means that the Darwinian model will not work for sociocultural phenomena" (p. 475). If Leeds is correct as to what modern social scientists do believe, then he is placing them as still in the intellectual climate of the British 1840s. Then, too, Christian and Romantic intellectuals thought that both apparent goal-directed activity and nonutilitarian behavior norms ensured that mankind could never be dealt with in

the same fashion as the rest of nature. Darwin does have answers for these problems, but they are often simply ignored. For example, they do not occur in Leeds' account of what "the Darwinian model" is. This reminds one of a major gap in the volume: there is no satisfactory account of what Darwin's set of presuppositions and theories actually was.

If I rated Montgomery as first because I think his procedure is needed, I have saved my two favorites in content for final mention. Thomas Glick's essay on Spain is so full of a number of things about the Spanish intellectual scene that it is pedantic to quibble over trifles. When he says that the "diffusion of evolutionary ideas in the provinces was, both for depth and for rapidity of permeation, remarkable" (p. 324), one can ask what his standard of comparison is. But this is the kind of thing that can be worked out later. Darwinism and Spain are now on the map, so to speak. Even more startling is Roberto Moreno's essay. One might have thought that there was nothing much worth saying about Darwinism in Mexico. But Moreno has provided an elegant analysis, with such interesting variants as that Darwin was known more from *The Descent of Man* than from the *Origin of Species*, that Mexicans relied principally on French sources, that anthropology was the science most affected by Darwin's influence, etc. There is more to be known about the history of science everywhere than one might suspect, if one will only look for it as Moreno has done.

W. FAYE CANNON  
Smithsonian Institution

WILHELM BACKHAUS. *Marx, Engels und die Sklaverei: Zur ökonomischen Problematik der Unfreiheit.* (Geschichte und Gesellschaft.) Düsseldorf: Pädagogischer Verlag Schwann. 1975. Pp. 267.

Because Wilhelm Backhaus' first concern is with Marx's analysis of slavery in the ancient world, he opens with a critical survey of early Soviet attempts to adjust the subject to the Procrustean requirements of the Marxian concept of class conflict and the operations of the dialectic.

As Engels stated retrospectively in 1886, Marx "recognized the historical inevitability, therefore the justification of the ancient slave owner, medieval feudal lords," etc., as "levers of human development for a limited period of history" and therewith the "temporary historical justification of exploitation, the appropriation of the products of labor." But Marx "proved" that such justification ceased when their continuation reached a point where it hampered "social" development and created "ever sharper conflicts." The Marxian

"proof" that this was the case in the Greek and Roman world, however, was extremely sketchy. Therefore, almost inevitably, the greater part of Backhaus' text is confined to the slavery question in the United States—the Civil War and various interpretations covering different aspects of the slave economy. The conflict in America was of immediate interest to Marx and Engels, coinciding as it did with serf emancipation in Russia and signs of unrest elsewhere in Europe, as portents of the approach of wider "social" crises that their theories envisioned and that they expected to experience directly.

In dealing with the American slave question, Backhaus offers a balanced account of the varying interpretations of the problem. He includes statistics on the slave population and its increase, the operations of the slave economy, slave labor on the land and in nonagricultural trades, competition between slave and free labor, and the contempt for physical work that the existence of slavery avowedly induced. Backhaus notes that Marx relied mainly on J. E. Cairnes' *Slave Power* (London, 1862) as a source of information, though he tended to ignore data that conflicted with his basic assumptions.

OSCAR J. HAMMEN  
University of Montana

V. G. KIERNAN. *Marxism and Imperialism.* New York: St. Martin's Press. 1975. Pp. xi, 260. \$18.95.

This excellent collection of essays is ideally suited for student use while informing the scholar as well. The author, V. G. Kiernan of the University of Edinburgh, though a Marxist, is no ideologue. He evaluates classical Marxist theory as it relates to the realities of imperialism in prose that is calm, informative, and often persuasive, even to those who start from a different perspective. Of the seven essays which comprise the book, six have appeared elsewhere—in *The Socialist Register* and the *New Left Review*—and their original venue may account for some of the reasonableness, in that they act as a powerful check to knee-jerk socialism. No other volume of which I am aware presents the basic Marxist interpretation in a way that all may understand.

The first essay, "The Marxist Theory of Imperialism and Its Historical Formation," is a substantial contribution to the literature, being lengthy (68 pages), carefully argued, and solidly documented. Another essay, "Imperialism, American and European: Some Historical Contrasts," is a sophisticated exercise in comparative analysis without sinking into a morass of model building or ascending to the heights of condemnation. It is not



unfriendly to the United States, and its concluding sentences represent the quality of the book and the direction of its commitments: "Peace and fraternity, America's early watchwords, have never lost all their virtue. If they do, if there is to be an ultimate failure of democracy in its first homeland, it will be a misfortune for the world only less great than an ultimate failure of the first homeland of socialism" (p. 131).

The final three essays, on the Labour Party and India, while somewhat less substantial than the rest of the book, also reach an equally interesting and perhaps more acute conclusion: "With Bertie Wooster to think was to act; with the Labour party very often to act was to think. I think, therefore I am" (p. 254). P. G. Wodehouse might not have been used to this company, or indeed welcomed it, but it is the measure of the book that it understands the human, and the humorous, dimensions of that bogey word, "imperialism."

ROBIN W. WINKS  
Yale University

A. E. MUSSON. *Trade Union and Social History*. London: Frank Cass; distrib. by International Scholarly Book Services, Portland, Ore. 1974. Pp. ix, 211. \$15.00.

The title of this book is somewhat misleading. It concerns primarily the history of organized labor and is only remotely related to general social history. One can conclude from these studies that it is difficult, if not impossible, to extract social history from the documents of trade unionism. All but one of these essays have been previously published as magazine articles or as portions of the author's published books, with the majority having appeared in print during the 1950s. Four of the nine essays pertain to the history of trade unionism in the early nineteenth century, which culminated in the formation of the Trades Union Congress in 1868. Only Chapter IV, which is a quick survey of modern trends, touches on trade unionism in the twentieth century. The author's detailed studies of the craft unions are designed to refute the ideological historians who insist on the solidarity of the working classes and the revolutionary character of the labor movement. The author contends that many trade unions are conservative and remain deeply committed to the traditions of their separate craft unions. In addition to the history of trade unionism, the book has four chapters on such diverse subjects as employers' associations, freedom of the press, the ideology of cooperation, and the early factory system. But these fragments of information contribute little to an understanding of

complex subjects that have been more thoroughly treated elsewhere.

R. G. COWHERD  
Lehigh University

ROGER H. STUEWER. *The Compton Effect: Turning Point in Physics*. New York: Science History Publications. 1975. Pp. xii, 367. \$25.00.

The Compton Effect was discovered in 1922 in the following way. By 1920 physicists based any acceptable theory of the nature of matter on the atomic hypothesis. Atoms were no longer unanalyzable entities; they contained two hypothetical entities: protons and electrons. At the same time fierce debate raged around the nature of electromagnetic radiation. Physics communities no longer treated such radiation solely as a wave phenomenon, yet one of the parameters used to characterize such radiation was the wavelength. Physicists realized that when electromagnetic radiation interacted with matter rich in electrons, the emerging radiation not only suffered a change in direction, but also underwent a change in wavelength. Generalizing from this phenomenon, A. H. Compton found it convenient, indeed, he found it compelling, to treat the interaction as one might treat colliding billiard balls. Thus came the name Compton Effect. Compton's work was one of the milestones along the road to quantum mechanics. For his efforts, Compton shared with C. T. R. Wilson the 1927 Nobel Prize in physics.

Stuewer recounts the history of Compton's discovery and shows why it was a crucial aspect of the history of physics. Standard treatments of the development of this part of twentieth-century physics suggest that Compton was simply playing out the consequences of Einstein's theory of the quantum nature of radiation, first proposed in 1905. As with most of Einstein's ideas, almost no one at the time had understood what he was talking about. Most colleagues considered him either incompetent or mad. When Einstein's theory received strong experimental confirmation nine years later, Milikan, the man who did the experiment, was surprised and even dismayed at the outcome. Subsequently he tried to divorce the result from Einstein's theory. As Stuewer takes pains to show, Compton did not come to his insight out of an understanding of Einstein's work; he probably never even read Einstein's theory.

The fact that textbook accounts of the connection between the work of Einstein and Compton are misleading is neither surprising nor unusual. Textbook accounts of the history of physics are almost always fairy-tales designed to ration-



alize the present. What is surprising is that Stuewer, reflecting a lack of historical imagination, seems dismayed by such distortion. His history of the event is accurate, but it is dull. Much of the text recounts already-published historical accounts of related events. He devotes enormous amounts of space to mathematical derivations that serve no historical purpose. They may reassure Stuewer that he understands physics; they may make the book appealing to physicists; but they do not belong in a genuine history of physics.

In his introduction Stuewer writes of his intention to focus on conceptual issues rather than social and cultural ones. The problem is that these conceptual issues are intimately entwined in a single social and cultural skein, the separation of which leads to an unraveling of the entire skein into a number of weak strands.

STANLEY GOLDBERG  
Hampshire College

JOHN T. BLACKMORE. *Ernst Mach: His Work, Life, and Influence*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xx, 414. \$16.95.

Ernst Mach is a pivotal figure in the history of early twentieth-century science. Most know of him for his significant contributions to the study of physics and psychophysics, but some also consider him the father of many of the current schools of philosophical empiricism. This is the first comprehensive view of Mach's life, life style, and work. That in itself makes it an important book.

Blackmore fulfills his stated purposes: 1) He presents as much new biographical material on Mach as possible in order to make him more familiar to "the educated public." 2) He clarifies the extent and range of Mach's contributions. 3) He brings attention to the degree to which Mach was a central and controversial influence on developing ideas in twentieth-century physics and philosophy of science.

As a child Mach had a great deal of difficulty assimilating visual stimuli. He had trouble, for instance, comprehending perspective and shading in paintings. All of his life Mach considered perspective art to be misleading and unnatural. Blackmore shrewdly relates this to the fact that to the end of his life Mach maintained that "understanding" in science was synonymous with ordering and assimilating sense experiences, perhaps with mathematical descriptions, but without the encumbrance of such undefined hypothetical entities as atoms or theoretical structures as the kinetic theory.

In spite of sharp intellectual disagreements with Ludwig Boltzmann over the question of the valid-

ity of the atomic theory, Mach and Boltzmann held each other in high regard and not only valued their interactions as opportunities to learn from each other, but actually shared in a friendly fashion the same platform in public debate. In short, the biographical material that Blackmore provides adds an important dimension to our understanding of Mach's work and influence.

Though Blackmore may have fulfilled his stated purposes, the book has two serious shortcomings. Blackmore informs us that the original manuscript from which the biography was written was twice as long. In the process of cutting, Blackmore allowed his prose to become obscure. And while I felt that much had been chopped out, I sensed also that the material which remained had not been sufficiently edited. The chapter on Mach's influence on philosophy, for example, degenerates into a mere listing of those persons in philosophy who had read Mach and were willing to say so in public.

But the most serious problem with the book is Blackmore's failure to identify his audience. Just who is "the educated public?" At times it must certainly be well versed in physics, but at other times it must be told the formal definition on entropy. At times, it must know a great deal of philosophy, but at other times Blackmore feels the need to dwell *ad nauseum* on formal definitional distinctions between philosophical schools.

In spite of these shortcomings, Blackmore's biography of Mach is a useful and needed addition to the library of the historian of science. I, for one, extend him thanks for having undertaken and completed this awesome job.

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II. STUART HUGHES. *The Sea Change: The Migration of Social Thought, 1930-1965*. New York: Harper and Row. 1975. Pp. x, 283. \$10.00.

In 1958 Stuart Hughes created a magisterial model of intellectual history to reveal with elegance, analytical precision, and methodological rigor the tensions between ideas and society. *Consciousness and Society* defined the major styles of twentieth-century thought as they developed concretely from 1890 to 1930 in an intellectual universe increasingly divided between reason and emotional value. Then, *The Obstructed Path* studied the generation of French thinkers who after 1930 turned from a national tradition morally and intellectually exhausted to a formalistic anthropology that left subjective and objective experience still sundered. In *The Sea Change*, his most personal book, Hughes concludes his remarkable revelation

of the roots and nature of contemporary thinking. The dual tradition bequeathed by Freud and Weber is resolved finally by the Central European émigré who combined, as does Hughes himself, scrupulous scholarship, intellectual honesty, and passionate ethical commitment.

*The Sea Change* is a psychosocial analysis of the effect of emigration upon the form and content of the émigré's social thought. Hughes concentrates upon the German-speaking intellectual who carried into exile doubts about the continuing validity of both "bourgeois humanism" and traditional culture or "Geist." The first chapter accepts the now conventional view that contemporary sociology and psychoanalysis resulted from a synthesis of German theory and Anglo-American empirical research, but the second argues that Wittgenstein in England, like Horkheimer, Adorno, Marcuse, and Tillich in America, were able to mediate between the two traditions only by refusing assimilation into their host countries. In the next chapter the enduring quality of Gaetano Salvemini's and Franz Neumann's critiques of fascism is attributed to their moral indignation, a fruitful use of broad Marxist categories, and a punctilious respect for evidence. Chapter four describes the preindustrial philosophical origins and the American isolation of the émigré critic of mass society who launched a precarious, but telling, cultural assault against both enlightened values and positivism. A fifth chapter treats ego psychology as the Freudian "Center" between the permissive "Left" of William Reich and Marcuse and the conformist "Right" of the neo-Freudian revisionists. In the concluding section, Hughes contends that there was an enriching exposure to Anglo-American thought only in those disciplines where nuanced language was unimportant. The half-understood, speculative émigré remained tied to his own European tradition and escaped solely in speculations on language and value. Émigré thinking, in its final effect, supports a conviction that neither Freud nor Weber would have accepted, but that Hughes finds correct: no individual's work can ever be either absolutely objective or value-free.

Hughes' richly textured, intellectually exciting trilogy offers a sustained and convincing explanation of our contemporary store of moral and intellectual concepts. In this final volume the crucial contribution of the embattled émigré becomes clear.

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ANTHONY DE CRESPIGNY and KENNETH MINOGUE, editors. *Contemporary Political Philosophers*. New York: Dodd, Mead and Company. 1975. Pp. xvii, 296. \$6.95.

As testimony to what they believe to be the vigorous state of political philosophy Anthony de Crespigny of the University of Cape Town and Kenneth Minogue of the London School of Economics have edited a collection of original essays by British and North American scholars on a dozen living political philosophers who have done their most important work since 1945. Six of the twelve thinkers are originally from the German-speaking world (Arendt, Hayek, Marcuse, Popper, Strauss, Voegelin), all of whom save Popper have taught and lived extensively in the United States. Three are French (Aron, DeJouvenel, Sartre), one is English (Oakeshott), one, Canadian (Macpherson), and John Rawls is American. The treatments are of an introductory kind designed to guide interested readers in their future explorations. Beginning with a brief introduction by the editors who deny the ideological, partisan, and prescriptive nature of authentic political philosophy, a position open to considerable debate, the volume concludes with biographical and bibliographical notes.

A number of problems are raised by the anthology. The first and obvious one is that of selection. Why is no one included from Eastern Europe, Italy, and the Third World? The left is far outnumbered by the liberal center and right, an inequality explained by the editors' reluctance to include what they term ideologists of a low intellectual level, although they admit that Marxists are "on occasions, formidably intelligent" (p. xiv). This criterion of selection seems to involve a most ideological notion of ideology, for surely Louis Althusser and Lucio Colletti are no more or less ideologists than, for example, Arendt, Hayek, Oakeshott, Strauss, and Voegelin, and at least as "formidably intelligent." Some compensation, however, for the ideological imbalance is the lengthy piece on Marcuse as a "representative figure," an excellent analysis by David Kettler. The choice of Popper instead of Isaiah Berlin is puzzling. Moreover, what is the justification for Aron's inclusion, especially since Ghita Ionescu throughout his able commentary insists upon referring to him as a political scientist? The essays are of an uneven quality, some being no more than basically uncritical eulogies—Dante Germino on Voegelin, Minogue on Oakeshott, De Crespigny on Hayek, E. F. Miller on Strauss—while others are competent evaluations—Anthony Quinton on Popper, Noel O'Sullivan on Arendt, Maurice Cranston on Sartre, and Samuel Gorowitz on Rawls. Only the slightest indication is given anywhere of the sizeable critical literature on most of the philosophers.

One must ask, of course, whether any of the twelve will be remembered in a few decades. It seems clear that some of them are largely the creatures of the whims and fancies of intellectual

fashion. What activist or student of political thought now pays much attention to Walter Lippman or Leonard Hobhouse who were so prominent a generation ago? Certainly Sartre will still be widely read in the twenty-first century, but how many of the others will only be studied, if at all, by curious historians seeking to map the culture of the age?

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PETER N. STEARNS. *Lives of Labor: Work in a Maturing Industrial Society*. New York: Holmes and Meier Publishers. 1975. Pp. viii, 424. \$28.00

The title of this book conjures up an expectation that is unfulfilled. *Lives of Labor* does not contain E. P. Thompson's compelling and sensitive exploration of working class experience; neither does it offer the brilliant analysis one finds in Eric Hobsbawm's work; nor does it present the exhaustive documentation and the high level of theoretical discussion which distinguish a number of recently published French theses. The book lacks many of the attributes often associated with social history. It is informed neither by the rigors of systematic quantification nor by the insights about culture and human behavior of social scientists.

There are a number of tables and a statistical appendix. The tables, however, are often difficult to decipher. The sources of the data are rarely cited and the methods used to extract information from censuses are never specified. More important, however, many of the data are unrelated to the major argument of the book. Their presence lends an aura of precision and scientific accuracy to what is in fact a highly tentative and often carelessly thought-out argument.

The argument focuses on "the culture of work." Yet Stearns makes no attempt to define that term. He gives no hint of a theoretical discussion of the components of a "culture," no suggestion of how to analyze it. He indicates that he does not employ literature, songs, and theater (pp. 7-9), but that section is really a discussion of sources, not an attempt at conceptualization. Without a clear concept of culture, the argument founders. The evidence presented seems contradictory, for the reader is at once told of the diversity, complexity, and conflicting outlooks of different groups of workers, and also of the presumably homogeneous "culture of work" they evolved.

The historical aspect of the argument is also troubling. In essence, Stearns insists that in the two decades before World War I, workers developed some new but highly ambiguous attitudes about work. Their expectations for "job satisfac-

tion" were higher than those of their predecessors, in part because of "increased job choice." These workers failed, however, to create institutions which would either secure job satisfaction or compensate for its absence. Their adaptation to "mature industrialization" involved neither complete alienation nor total fulfillment. "The legacy" of the period was an "enduring tension between hope and reality" (p. 352).

This argument rests on an implied comparison with an earlier period, presumably the period of "immature" industrialization. Nowhere, however, is the comparison documented, nowhere are the differences spelled out. What *was* distinctive about the period from 1890 to 1914? Why not 1870 to 1914, or 1906 to 1936? In addition, many of Stearns' descriptions of changes in work pace, the fear of unemployment, and the displacement caused by technology apply to the early or mid-nineteenth century as well. His evidence of increased job choice comes primarily from public opinion polls, a measure of attitude which did not exist until this period. There is no evidence, in other words, that attitudes have really changed.

"Job choice," as Stearns uses the term, implies a desire for satisfaction at work and the freedom to choose among many options. It is a mid-twentieth-century notion of psychic fulfillment that has limited application even at the beginning of this century. "There were good reasons to choose textile work," says Stearns, "especially in places like Lancashire where parental example was obvious" (p. 71). Contrary to Stearns' implication, however, the "good reasons" had little to do with parental example or free will. No other work but textiles was available or paid as well in Lancashire. And that is why young people "chose" the same jobs as had their parents and probably their grandparents.

The case for "increased job choice" is flimsy and so is much of the rest of the book. Stearns' indictment of the labor movement for its preoccupation with wages and hours seems far-fetched. It distorts the reader's understanding of the relevant issues of the period, and it profoundly misinterprets the experience of working people at the turn of the century.

Above all, the book lacks discipline. The implications of the entire argument have not been worked out thoroughly. The evidence is not well-organized. Terms which demand definition ("maturation," "sophistication") are undefined. Even the sentences are often clumsy and unclear. *Lives of Labor* is a disappointing book. It does not fulfill the promise of its title, nor does it do justice to the importance and interest of its subject.

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ROBERT A. KLEIN. *Sovereign Equality among States: The History of an Idea*. Toronto: University of Toronto Press. 1974. Pp. xix, 198. \$10.00.

The appearance of Robert Klein's brief history of the idea of equality among nation states must be described as "timely" and "appropriate" in view of daily headlines reporting combinations of small states outvoting power, size, and wealth (usually the United States) or isolating other small states (usually Israel) at international assemblies. Klein's book reproduces the nineteenth- and twentieth-century background which led up to the current confrontation between the tradition of great power primacy, on the one hand, and the challenge posed by the more democratic, more chaotic "one nation, one vote" rhetoric and practice, on the other. Both Klein and Louis J. Halle in his generous foreword, share the intellectual persuasion that the idea of sovereign equality reflects a radical change in human thinking about international relations, a change in consciousness which, Klein argues, can be traced to episodes in inter-American relationships during the last decades of the preceding century, to the Wilsonians, and to the practices of regional associations of states in the twentieth century. Klein adheres to that school of intellectual formulation which believes in the moving power of ideas that have grown from seedlings to mature plants. His chapters cover the appearance of the idea of sovereign equality among nation states to sources in domestic legal forms, to tenuous roots at the Congress of Vienna, through occasional speeches at Latin American and European conferences in the pre-1914 generation, and then into the more familiar territory of the League of Nations, the FDR years of neighborliness, and the creation of the United Nations. Specialists in any of these epochs will recognize the names, events, and interpretations.

This study is essentially a restatement of the headaches created by the introduction of liberal democratic ideas by the western upper bourgeoisie into transatlantic societies during the late eighteenth century. From that point, both in the domestic and international order, the coupling of the word "equality" with "liberty" produced "discord" to quote Klein's major plaint about the new international order. "Discord" or contradictions must result from the utter impossibility of achieving equality and freedom simultaneously, a contradiction which many western liberals have finally learned to face. Restructuring, resurrecting, and defending great power responsibility in the international order may be a Band-aid solution to put off the day of reckoning upon another generation. Klein offers the impression that he knows

this unpleasant truth, but wishes it would go away.

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ARTHUR MARWICK. *War and Social Change in the Twentieth Century: A Comparative Study of Britain, France, Germany, Russia and the United States*. New York: St. Martin's Press. 1975. Pp. x, 258. \$16.95.

The subject of this essay in comparative history is the impact of the world wars on Britain, the United States, France, Germany, and Russia. There are four major themes: the destructive effect of war; the testing of societies by war; the participation of various groups in the wars (for example, the rise of the working class—which results from its usefulness in wartime); and the psychological impact of war. Arthur Marwick refers to these themes as his "four-tier model." All of this sounds very sociological, but in fact the author does not allow his "model" to dominate the presentation. Indeed, his history crowds out his sociology, and the "model" ends up being little more than a collection of useful topic headings; but there is nothing wrong with that.

Marwick emphasizes the use of film and fiction as sources for social history. There is a whole section devoted to archival film material. His annotated bibliography, notes, and index are all useful, and the plates are of considerable interest. The text is easy to read, and his judgments are quite sound. This will be a useful book not only for graduate students in military or social history, but also for upper level undergraduates—that is, if it comes out in a low-cost edition. The specialist, however, may find *War and Social Change* of limited value. Although it is nice to have so much statistical and bibliographical information in one volume, the book is brief and largely based on secondary sources. There is little new. Marwick is sensitive to the problems of methodology and causation, but his remarks on these topics—indeed, on all topics—are sketchy and hurried because he has so much to cover. Readers will be particularly interested in the author's comments on the intellectual and psychological aspects of his subject, though these too were frustratingly brief.

Marwick's conclusion is a relatively happy one: the human race has not only survived its world wars—so far—but it has also made progress, both despite and because of those terrible wars.

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J. BOWYER BELL. *The Myth of the Guerrilla: Revolutionary Theory and Malpractice*. (Written under the auspices of the Center for International Affairs, Harvard University.) New York: Alfred A. Knopf. 1971. Pp. xiv, 285, ix. \$7.95.

JOHN DUNN. *Modern Revolutions: An Introduction to the Analysis of a Political Phenomenon*. New York: Cambridge University Press. 1972. Pp. xiii, 346. Cloth \$14.50, paper \$4.95.

MICHAEL ELLIOTT-BATEMAN *et al.* *The Fourth Dimension of Warfare. Volume 2, Revolt to Revolution: Studies in the 19th- and 20th-Century European Experience*. Towata, N.J.: Rowman and Littlefield. 1975. Pp. 373. \$21.50.

These three studies are contributions to the rapidly expanding literature on what are variously called "guerrilla revolutions," "people's wars," or wars of "national liberation" in the twentieth century. All use the case study method. The authors agree on the nature of the phenomenon: the conjuncture of war and revolution in a largely peasant population, the preponderance of areas in colonial or semicolonial relation to the West, and the emphasis not so much on the conquest of the battlefield as of the mind—that is, the erosion of the will of the stronger and the development of an energizing myth capable of sustaining revolutionary ardor no matter what the sacrifice. Nevertheless, they differ in matters that concern origins.

J. B. Bell is concerned with the new generation of guerrilla fighters that appeared in the 60s. For perspective, he traces the use of guerrilla tactics from its first great "modern" success, the Irish Republican Army's Easter Rebellion of 1916, to Mao in the 30s, the World War II weakening of colonial powers, Dien Bien Phu, and corollaries to the general theory by Fanon, Castro, and Guevara. With Guevara's new "myth of the guerrilla," the fantasies of the "desperate and naive" now swept into fashion "over the old orthodoxies of political mobilization, parliamentary infiltration, trade-union militance, and balanced insurrections" (p. 42). Bell makes this new current the subject of three case studies: the wars of liberation in southern Africa, the terror tactics of the Palestine Fedayeen, and the attempt of Guevara to establish "another Viet Nam" in Bolivia—all substantial failures. He feels the "myth of the guerrilla" is out of touch with reality. The attempt to transform tactics into strategy, the notion that "repeated failure paid repeated benefits," has proved to be not only "ineffectual but often self-defeating" (p. 101).

A work of political theory, John Dunn's study also ends on a pessimistic note. The eight case studies from the Russian to the Cuban revolutions

are all examples of successful overturns of governmental authority. Five revolutions have in some sense been communist (the Russian, Chinese, Yugoslav, Vietnamese, and Cuban, though the latter "very much in retrospect"), and the communist revolutions affected the other three (the Mexican, Algerian, and Turkish revolutions). The bias of the examples, in contrast to Bell, is toward regimes that have attempted to establish an order of social relations inspired by Marxist ideas. Dunn agrees with Bell that twentieth-century revolutions are not based on Marxist theory, but he notes a "core of truth" in the Leninist theory of imperialism: these revolutions have taken the form of wars of national liberation, often from a colonial yoke. He concludes that revolution is "a metaphor of the recapture of control by the virtuous over an inimical destiny" (p. 255). In many ways it is a feeble remedy for the ills of some societies and "no remedy at all for many of them" (p. 257).

The general evolution of guerrilla warfare to its present advanced state is the theme of the Elliott-Bateman collection of essays. In contrast to the arguments of Bell and Dunn, its "modern phase" extends at least as far back as the French Revolution, though only since 1945 has there been an alarming acceleration of guerrilla activity, thereby introducing the "age of the guerilla [*sic*]" (p. 7). Ellis assesses select European revolutions since the Puritan to delineate the "guerilla-revolutionary marriage." He concludes: "The doctrines of a Mao or a Giap should not be regarded as a qualitative breakthrough in militarized revolution but rather as a more successful attempt to grapple with problems that had already been faced, and sometimes solved, in previous centuries" (p. 144). T. E. Lawrence's brilliant essay on the Arab revolt of 1916 and several essays on the Irish revolutionary experience by Foot and Bowden are also included. The closing essays by Elliott-Bateman on the "conditions" and the "battlefronts" of a people's war are designed to establish a "theory of this form of modern warfare."

All three studies have elements of strength, but they are not without shortcomings. Bell's concluding analysis of the "myth of the guerrilla" is perhaps as solid as anything in the literature, though he strangely neglects Georges Sorel's study of revolutionary myths. Dunn demonstrates a keen sensitivity to "Leninism," but he might have explored its application further in the case studies he cites. The case studies in Elliott-Bateman on the I. R. A. make exciting reading, though judgments concerning "conditions" and "battlefronts" often result in clichés or generalizations that require such qualification as to make the result of little or no value. But all three studies (with the possible



exception of Dunn's) are on uncertain ground when they try to establish "origins" or "revolutionary traditions." The shift in the twentieth century of the center of revolutionary change from the "core-countries" of Europe to those in colonial or semicolonial relation to Europe, a shift from urban to agrarian, from Western to anti-Western would appear to be of overriding importance.

The shift is instructive, because it also points to the centrality of the Bolshevik Revolution for the most important of these movements. The link is not Marx's pseudo-scientific theory of society. It is rather Lenin's model of a revolution which was, as Dunn rightly asserts, "elitist, manipulative, and unrelentingly opportunist." The semi-colonial status of Russia, the preponderance of a peasant population, and the anti-Western aspects of the civil war and intervention established the relevance of the Bolshevik Revolution for a variety of movements that followed, military techniques notwithstanding. Though innumerable qualifications may come to mind, the revolutionary wars of the twentieth century are related to the Bolshevik Revolution as nineteenth-century revolutionary disturbances in Europe were related to the French Revolution.

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*Essays in Honour of E. H. Carr.* Edited by C. ABRAMSKY. Hamden, Conn.: Archon Books. 1974. Pp. viii, 387. \$15.00.

*Festschriften* are notoriously difficult to review. Collections of articles tend to be different in content, disparate in quality, and altogether impossible to evaluate in a page or two. Without rising above its genre, the present volume, which was published to commemorate E. H. Carr's eightieth birthday, fortunately has more useful material and even more cohesion than many other similar works. Although some studies contained in it are clearly better than others, none is a total loss or in apparent need of wholesale correction. The several distinct parts of the book reflect the main aspects of Carr's huge scholarly activity, while they offer at the same time interesting evidence of contemporary British scholarship, particularly in the Russian field.

Part One, "Intellectual History in the Nineteenth Century," consists of four contributions: Isaiah Berlin's sparkling study of Georges Sorel; Monica Partridge's close examination of Alexander Herzen in his last phase; Arthur Lehning's exposition of "Bakunin's conceptions of revolu-

tionary organizations and their role"; and G. A. Cohen's logical analysis of the relationship between being and consciousness in Marxism. Part Two, "Diplomatic History and International Relations," adds another four contributions by four different authors. Beryl J. Williams writes on "the Revolution of 1905 and Russian foreign policy"; Eleonore Breuning on "Brockdorff-Rantzau: the 'wanderer between two worlds'"; and D. C. Watt on "the initiation of negotiations leading to the Nazi-Soviet pact." Finally, Roger Morgan concludes the part with an appropriate and not at all uncritical discussion of "E. H. Carr and the study of international relations." Part Three, "Soviet Studies," is the longest one in the book, some 160 pages compared to about 100 in Part One and about eighty in Part Two. Six new authors make their contributions. Alec Nove offers some observations "on Bukharin and his ideas"; John Erickson deals with certain "military and political aspects of the 'militia army' controversy, 1919-1920"; Michael Kaser draws some statistically grounded comparison between tsarist and Soviet education. Next, R. W. Davies and Moshe Lewin write on closely related themes: the first on "the Soviet rural economy in 1929-1930: the size of the Kolkhoz," and the second on "Soviet policies of agricultural procurements before the War." The sixth author, Maurice Dobb, engages in "some historical reflections on planning and the market" instead of delving into a more circumscribed research topic. The fourth and last part, "Related Studies," is the shortest and serves as something of a grab bag. The two contributors, Mary Holdsworth and Lionel Kochan, discuss respectively "Lenin's *Imperialism* in retrospect," and "Graetz and Dubnow: two Jewish historians in an alien world." In addition to the sixteen studies, the volume contains a frontispiece photograph of E. H. Carr, a brief tribute to him by Abramsky, a bibliography of his works by Beryl Williams, notes on the contributors, and a general index.

The *Festschrift* is obviously rich in content and quite well done. The most interesting studies turned out to be Berlin's perceptive analysis of Sorel's thought, which emphasizes its striking relevance to the present day (a version had been published in the *Times Literary Supplement* of December 31, 1971), Watt's probing search for the exact time of the initiation of the negotiations leading to the Nazi-Soviet pact, and Lewin's expert treatment of Soviet agricultural procurement, which presents better than any other short piece the tragedy of the Soviet peasant.

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## ANCIENT

DAVID DAUBE. *Civil Disobedience in Antiquity*. Edinburgh: Edinburgh University Press; distrib. by Aldine-Atherton, Chicago. 1972. Pp. xii, 167. \$7.50.

Civil disobedience proved to be an effective tactic of opposition in the sixties for groups excluded from the establishment in American society. In a series of public lectures delivered at Cornell University in 1971, David Daube, a distinguished legal scholar, appropriately focused attention upon similar dissent by disaffected social classes and minorities in antiquity. These lectures are now published in a small book of six chapters, dealing with women, the young, slaves, prophets, philosophers, religious minorities, and groups aspiring to statehood.

Adopting the lawyer's definition of civil disobedience as breaking of the law by nonviolent means, in which the perpetrator is willing to undergo the punishment prescribed by it, Daube explores his theme broadly and is able to maintain an interesting balance in each category of familiar and less-known cases from the Judaeo-Christian, Greek, Hellenistic, and Roman world. The Bible and writings from ancient Greece provide the best-known examples of nonviolent disobedience, but one is struck by the degree to which the Roman dissenters are less likely to be known to the intelligent layman. The secession of the plebs in the Early Republic is familiar enough from Livy, but Daube digs deeper to recall the publication of the *libri pontificum* and those Roman matrons whose extravagance prompted the Oppian Law and denunciation by the elder Cato.

Written in a chatty, casual style, the book, with its wealth of examples, develops historical perspective on a subject of current concern.

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## MEDIEVAL

JOSEPH F. O'CALLAGHAN. *A History of Medieval Spain*. Ithaca: Cornell University Press. 1975. Pp. 729.

A timely manifestation of the new interest in medieval Spain among English-language scholars, this book is a comprehensive narrative history based on an impressively wide reading in the sources and secondary literature. It should prove useful to teachers, students, and general readers in European history, although it is possibly too long—and, in its present form, too expensive—to serve as a required textbook. Soundly traditional in its organization, it gives primacy to political

events without neglecting institutional, social, and cultural matters. It may be said, on the whole, to have supplanted the pertinent chapters in the *Cambridge Medieval History*, being equally readable and much more nearly up-to-date.

Among the book's substantive merits may be mentioned its ample discussion of the Visigothic period, which is ceasing to be negligible as it becomes better known; its vigorous and sympathetic treatment of the Muslim realms; and its insistent attention to Portugal. Joseph F. O'Callaghan has scrupulously avoided interpreting the early Conquest generations in a Christian perspective or the Reconquest ones in primarily Castilian terms; nor does he minimize the interest of the Moorish remnant-states and Mudéjar communities in the later Middle Ages. He tends to accept the theses of Menéndez Pidal and Sánchez Albornoz on institutional and ideological continuity and political and social reconstruction. His exposition of political and military events after 1085, if not always easy reading, justly appreciates the complications of purpose in the ruling classes of the many human groups which retained or achieved political identity in Reconquest Hispania. The powerfully subsistent idea of a single Spain was in brutal conflict with equally powerful impulses of a particularist or dynastic nature.

The book seems to this reviewer better on political and institutional topics than on social-cultural ones, an imbalance which may bear some relation to the prevailing state of the underlying scholarship. Spain's importance for the development of associative structures—towns, military-clerical orders and representation—is well brought out. Feudalism and "courtly love" are less successfully interpreted, partly, at least, because the author is insufficiently critical of the conventional but misleading labels for these complex phenomena. But the comparative study of vassalic and feudal relations in the Hispanic realms is only just beginning, and in this matter O'Callaghan could have found little conceptual stimulation in the works he has relied upon. The treatment of early Catalonia is not altogether secure. Vich and Cardona are not in the Segre basin (p. 106); nor is there any reason to doubt, for unequivocal evidence survives, that the Catalán and Aragonese towns were represented in the assembly of Lérida in 1214 (p. 437). Moreover, the emphasis on Catalonia's singularity in the comital period, a main point of this book and hitherto generally accepted, has been shown to be mistaken in recent work by Bastier, Bonnassie, and Zimmermann.

The studies just mentioned, however, were published only when the present volume was complete. The point in question is mentioned simply to

illustrate the major difficulty with this courageous work. So much basic research remains to be done on medieval Spain, and is currently being done, that a book of this kind must inevitably be provisional in important respects. But the book was needed now, and both author and publisher deserve hearty thanks for producing such a valuable survey. May they be encouraged to keep it up to date and in print!

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ANDREW HANDLER. *The Zirids of Granada*. Coral Gables: University of Miami Press. 1974. Pp. 208. \$10.00.

Like Renaissance Italy, eleventh-century post-Caliphal Islamic Spain was a welter of principalates distinguished for culture, intrigue, and bloody wars. Neglected until recently (Lévi-Provençal's magisterial history stops short; Prieto is relentlessly numismatic; and Menéndez Pidal illuminates fitfully), it has been yielding its secrets to determined researchers: Bosch Vilá (1959) on Albarracín, Huici (1970) and Sanchis Guarner (1965) on Valencia, Sosselló Bordoy (1968) and Busquets Mulet (1973) on the Balearics, Monroe on society, and Ashtor and Baer on the Jews, not to mention the literary and architectural researchers.

Now Andrew Handler, in his dissertation from Columbia University, has industriously sorted out the brief Berber sultanate of Granada (at the very time Arié and al-ʿAbbādī have covered its illustrious later namesake). The intractable sources limit him to a "political study," the mind-numbing but important sequence of battles, cruelties, revolt, and other drum-and-trumpet circumstance; a final chapter discovers Umayyad rather than African Zirid administrative-social patterns. Handler systematically interweaves such well-known and often later chroniclers as Ibn al-Abbār, Ibn Abī Zarʿ, Ibn Bassām, Ibn ʿIdhārī, Ibn Khaldūn, Ibn al-Khaṭīb, al-Maqqarī, and al-Marrākushī, but especially the memoirs-cum-history by ʿAbd Allāh b. Bulūqqīn, the last Zirid ruler. He also leans heavily on Lévi-Provençal, Idris, Dozy, Ashtor, and Baer. One feels unease at the somewhat restricted bibliography (neither Huici nor Bosch is there, for example, nor the posthumous Torres Balbás collection, nor, among the translations, Hoenerbach's 1970 Ibn al-Khaṭīb); but that is minor. Handler has done what he set out to do, in lucid and sober style, with the essential materials well researched and arranged—to fill the gap of a political history for Granada's Banū Zīrī.

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AZIZ AHMAD. *A History of Islamic Sicily*. (Islamic Surveys, 10.) Edinburgh: Edinburgh University Press. 1975. Pp. xi, 147. £2.50 net.

Muslim rule of Sicily began and ended with treachery or folly: in 827, the Byzantine rebel Euphremius called on the Tunisian Aghlabids as allies, offering them sovereignty over the island in exchange for the title of emperor for himself; almost a quarter of a millenium later, a desperate local *qā'id*, Ibn al-Ṭhumna, appealed to the Normans for help, in the foolish hope that they would return the island to him after having conquered it.

Even at the peak of Sicily's Islamic period under the Kalbite emirs, effective Muslim control remained uneven beyond the Val di Mazara in the southwest. The Greek population, concentrated in the northeast, clung tenaciously to its Christian identity. The Muslim majority—mostly Arabs and Berbers, with a sprinkling of Negroes, Persians, and Turks—was rarely without internal feuds and rivalries. Benevolent Norman rule brought out the best in the unique coexistence of two great cultural and intellectual traditions and made Sicily one of the major transmitters of Greek and Islamic learning to the Latin world. Yet in the same period we witness a steady decrease of Sicily's Muslim population through emigration and voluntary conversion. Ironically, it was the Hohenstaufen Frederick II, perhaps the greatest admirer of Islamic culture, who terminated the Muslim presence on the island. Resettled in the military colonies of Lucera and Grottefalcone on the Apulian plateau, the last Sicilian Muslims survived as a distinct ethnic group until the end of the thirteenth century. In 1300, they were forcibly converted by Angevin orders.

Aziz Ahmad gives us a compact, well-documented history of Islam in Sicily. If severe restrictions of space have reduced some passages to little more than enumerations of names and dates, a thirteen-page bibliography of Arabic and Western sources compensates to some extent for this shortcoming. Of the 283 items listed, a mere thirty-four are English titles, of which only thirteen are of immediate relevance. Nothing could demonstrate better the importance and value of this contribution.

KARL STOWASSER  
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STANLEY RUBIN. *Medieval English Medicine: AD 500-1300*. New York: Barnes and Noble. 1975. Pp. 232. \$15.00.

Slightly more than a century ago the Reverend Oswald Cockayne edited for the Rolls Series the monumental *Leechdoms, Wortcunning and Starcraft of*

*Early England.* Since that time most readers in medieval medical lore have been fascinated by the bizarre nature of what they assumed to have been standard medical practice in Anglo-Saxon times. No doubt magic, astrological calculations, and religious incantation belonged to the leech's art, and shocking, nauseous substances such as burnt mice, frog's blood, and ox's urine were among the ingredients of some medical recipes. Yet, by over-emphasizing such oddities, often to the exclusion of the more basic elements, many successors of Cockayne have served to denigrate all medieval English medicine.

Stanley Rubin, in his *Medieval English Medicine*, adopts a more realistic approach. Assuming that no art based mainly on nonsense can long endure, he shows that medieval English medicine was essentially rational, and that many of its practitioners were persons of compassion and good sense. Here he accepts the thesis of C. H. Talbot who a decade ago (*Medicine in Medieval England*) challenged some of the assumptions attributable to Cockayne's work. Though Rubin's book is a good introduction to the subject, he makes no claim to definitiveness. His use of primary sources is assuring, and he has described the more significant ones in appendices. Although he has established the year 1300 as the *terminus ad quem*, the Anglo-Saxon era receives the major emphasis. Here Rubin accepts the current thesis that English medicine in the ninth and tenth centuries was not inferior to that of its continental neighbors, since the best of classical medical knowledge was incorporated into Anglo-Saxon medical literature even before it became a part of the Salernitan tradition.

A laudable feature of the Rubin book is the opening chapter devoted to archaeological and paleopathological evidence, a source of data frequently overlooked. There is a splendid chapter on leprosy and society's attitudes toward it. In certain areas the paucity of sources necessitates cautious generalities, as is true in the matter of the development of medicine as a profession and in some of the features of monastic medicine. Rubin has resisted the temptation to read more into his sources than is warranted, and his interpretations are well considered and generally defensible.

E. A. HAMMOND  
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G. O. SAYLES. *The King's Parliament of England*. (Historical Controversies.) New York: W. W. Norton and Company. 1974. Pp. x, 164. Cloth \$7.95, paper \$2.95.

This brief book, which is based on the published and unpublished works of G. O. Sayles and H. G. Richardson, should command the attention of all

students of constitutional history. For half a century these indefatigable scholars have contributed to the understanding of the governance of medieval England. It is the simple truth that any discussion of medieval parliaments must start with a review of their findings.

*The King's Parliament of England* consists of seven essays, varying somewhat in length and degree of specialization. The first, "Modern Myths and Medieval Parliaments," sets the emphasis for the book. In spite of the misconceptions fostered by seventeenth-century polemicists and such nineteenth-century scholars as Stubbs, who studied the history of parliament in terms of what interested them, parliaments were not originally, and never primarily, representative assemblies preoccupied with legislation and taxation. They were courts rather than legislatures, their business primarily judicial; the absence or presence of representatives of the commons was irrelevant to their competence and authority. Sayles is gentle and patient with those slow to grasp this fact, which the records amply support. Yet he does get in a swipe or two at the History of Parliament Trust. "We only delude ourselves," he writes, "if we believe that an understanding of the medieval parliament can be reached by compiling biographies of members of the House of Commons." The interpretation is further developed in the succeeding essays, especially those on the Provisions of Oxford, the Parliaments of Edward I, and the Ordinances of 1311.

In the last essay, "The King's Parliaments in Perspective in the Later Middle Ages," Sayles discusses some of the changes which occurred as the judicial work of parliaments decreased and political matters became their major concern. He reminds the reader that as late as the reign of Henry VII, it was accepted that certain matters were for parliaments only, while other important questions came fully within the competence of great councils meeting without elected representatives.

The Appendix contains a list of parliaments from 1258 to 1377, an admirable glossary, of special utility for beginning students, and a useful bibliography.

CATHERINE STRATEMAN SIMS  
Agnes Scott College

R. H. HILTON. *The English Peasantry in the Later Middle Ages*. (The Ford Lectures for 1973 and Related Studies.) New York: Oxford University Press. 1975. Pp. 256. \$21.00.

R. H. Hilton's first concern in these Ford Lectures is to establish the position of the peasants as a social class. This he does in a five-point definition (p. 13). He studies the peasantry of the West Midlands—a region he has made his own—during the century after the Black Death. The peasant family

holding was usually one yardland or half as much. The typical peasant household included three generations, but the generation gap was narrow, and grandparents took care to make their own place secure before handing over. It was from this extended family that jurors, tithingmen, reeves, and ale-tasters were drawn. These notables were in *de facto* control of village affairs; the manor court enforced their authority.

In his lecture on "The Small Town as Part of Peasant Society" Hilton is not concerned with Coventry or Bristol, but with the scores of towns with 500 or fewer inhabitants, each "contained in a rural envelope of arable fields, meadows, and pastures." Despite their involvement with surrounding fields and pastures, "the weekly market was the focus of their lives" (p. 83). In "Women in the Village" Hilton suggests that peasant women did every kind of agricultural work and were paid as much as men. Customary law awarded widows more than common law, and there was less compulsion for widows to remarry than in the aristocracy.

Hilton presents his evidence in detail. Nearly all his references are to unpublished sources. Conclusions are infrequent, however, which is disappointing, for Hilton's acquaintance with the West Midland peasantry is unequalled, and one would wish for more of the fruits of his reflection.

The six lectures fill 110 pages and the rest of this small volume is given over to some of Hilton's related papers, "now difficult to obtain." They are "The Social Structure of Rural Warwickshire in the Middle Ages" (1950); "Gloucester Abbey Leases of the Late Thirteenth Century" (1953); "A Study in the Pre-History of English Enclosure in the Fifteenth Century" (1957); "Rent and Capital Formation in Feudal Society" (1962); and "Lord and Peasant in Staffordshire in the Middle Ages" (1960).

W. O. AULT  
Boston University

CHARLES ROSS. *Edward IV*. (The "English Monarchs" Series.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xvi, 479. \$25.00.

Charles Ross' book is the first full-length study of Edward IV since Cora L. Scofield published her two-volume "Life and Reign" in 1923. Scofield's book, a painstakingly minute narrative of politics and diplomacy, will remain valuable for its detailed information. It is intolerably dull, however, and suffers from the dearth of information on late fifteenth-century social and political conditions available at the time it was written, and it offers the minimum of interpretation. Ross' book, though far less detailed in its political narrative, is

grippingly readable. It presents a new, vivid synthesis based upon the author's own researches and upon his cool analysis of the work of the present generation of historians who, since about 1956, have vastly increased our understanding of the social and governmental structure of late fifteenth-century England. Its most characteristic contributions lie in Ross' comments on the royal finances, foreign policy, and the related subjects of the nobility, patronage, and the king's controversial marriage to Elizabeth Woodville.

Ross is skeptical of recent panegyrics of Edward's financial acumen. He accepts B. P. Wolffe's thesis on the importance of Edward's reformed administration of crown lands, but he rightly questions the high level of efficiency Wolffe claimed for the new system. Ross seems to press too far his doubts about the existence of the king's policy of estate accumulation. He takes insufficient account of the harsh, political realities of patronage, even bribery, which made it essential for a king, who had only just managed to seize the throne by force, to buy the support of powerful men.

Ross could have taken a more critical view of the effects of fiscal feudalism and the efficiency of the customs system. The famous comments of the Second Anonymous Croyland Continuator on the king's increasing financial severity may reflect little more than the resentment of feudal tenants and merchants who naturally disliked attempts to tighten control. For example, though Edward appointed strict supervisors of the customs in most of the ports, he never attacked the problem of scandalous undervaluation. Ross does not seem to be aware that the conventional customs valuation of many goods was as little as one-seventh of their true commercial value.

The author agrees with recent reassessments of the nobles' role—that they were drawn only with the greatest reluctance into the Wars of the Roses and that Edward never attracted the active loyalty of the peerage as a whole. Yet they and their affinities were vital for controlling the countryside, and Edward, creating a large number of new peers to support his throne, distributed his patronage with this in mind. In the end he failed because he distributed patronage too narrowly, and he alienated powerful families by arbitrarily interfering with the common-law rules of inheritance at their expense. Thus, in the last years of his reign politics became the preserve of about one-third of the nobility. This narrow basis of support, together with the universal (perhaps Ross exaggerates here?) unpopularity of the queen's family, the Woodvilles, left the peerage deeply indifferent to the fate of Edward's son and thus made possible the usurpation of Richard III. So we are left with the harsh verdict that Edward failed in the most important

task of a dynast: to secure the succession of his heir.

J. R. LANDER  
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### MODERN EUROPE

LAWRENCE STONE, editor. *The University in Society*. Volume 1, *Oxford and Cambridge from the 14th to the Early 19th Century*; volume 2, *Europe, Scotland, and the United States from the 16th to the 20th Century*. (Written under the auspices of the Shelby Cullom Davis Center for Historical Studies, Princeton University.) Princeton: Princeton University Press. 1974. Pp. ix, 352; viii, 355-642. \$12.50 each, \$22.00 the set.

This book of essays, a cornucopia of facts, figures, and interpretations, is a product of a research seminar held at the Shelby Cullom Davis Center for Historical Studies from 1969 to 1971. All thirteen contributors were participating members of the seminar, either as visiting fellows of the Center, as faculty at Princeton University, or as graduate students; and, as Lawrence Stone, Dodge Professor of History at Princeton and editor of this book, points out, "in one way or another they are all interested in the relationship between formal education and other social processes, rather than with either [*sic*, ed.] the history of educational institutions as such, or with [*sic*] the history of changes in the curriculum and scholarship as such" (p. v). Their primary concern is with the sociology of education.

Volume I deals with developments in Oxford and Cambridge from the fourteenth to the early nineteenth century. Lawrence Stone presents data about the size and composition of the Oxford student body (1580-1909); Guy Fitch Lytle analyzes patronage patterns and Oxford colleges (c. 1300-c. 1530); and James McConica discusses findings about scholars and commoners in Renaissance Oxford. Victor Morgan examines Cambridge University and "the country" (1560-1640); Sheldon Rothblatt describes student subculture and the examination system in early nineteenth-century Oxbridge; and Arthur Engel writes about the emerging concept of the academic profession at Oxford (1800-1854).

Volume II is concerned with significant educational and societal developments in Europe, Scotland, and the United States from the sixteenth to the twentieth century. There are essays on a variety of topics: universities in Castile (1500-1810), by Richard L. Kagan; culture and society in the eighteenth-century province (the case of Edinburgh and the Scottish Enlightenment), by Nicholas Phillipson; American student societies in the early nineteenth century, by James McLachlan; univer-

sity reformers and professorial scholarship in Germany (1760-1806), by R. Steven Turner; the sources of German student unrest (1815-1848), by Konrad H. Jarausch; economists as experts and the rise of an academic profession in America (1870-1917), by Robert L. Church; and the values and goals of freedmen's education in America by James M. McPherson.

In general, the authors demonstrate commendable scholarship, productive approaches to research, and intelligent use of sources. Their interpretations are provocative without being dogmatic, and, with the exception of one contributor with a penchant for hyperbole, the authors' judgments are usually sound. The statistical data provided by Stone (pp. 3-110) and the perceptive essay by Sheldon Rothblatt (pp. 247-303) should prove to be of particular value to historians. Contemporary scholars, caught in the academic vise that, on one side, pressures them to teach, and, on the other side, pressures them to do research and writing, may also want to spend considerable time with R. Steven Turner's essay, "University Reformers and Professional Scholarship in Germany 1760-1806" (pp. 495-531).

In brief, although no attempt is made to synthesize these essays, the book reflects the achievements of a number of fine scholars.

DANIEL ROSELLE  
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MOSES A. SHULVASS. *The Jews in the World of the Renaissance*. Translated by ELVIN I. KOSE. Leiden: E. J. Brill. 1973. Pp. xv, 367. 58 gls.

This work was first published in Hebrew in 1955. The present English title is a misnomer, since the book deals only with the Jews of Italy in the period 1300-1600, and not with the Jews of the general European Renaissance.

Moses A. Shulvass' solid study shows how the Italian Jewish community developed from a relatively indigenous one centered in Rome to a mixed one, as refugees flowed in from France, Germany, Spain, and the Levant. Gradually three major communities developed: Italian, Ashkenazic, Sephardic. Local situations plus the variations in the traditions and practices of the groups led to different kinds of communities, with different economic activities, Jewish organizations, linguistic practices, etc. Shulvass gives a detailed and often fascinating picture of what Jewish life was like at this time, what its cultural achievements were, and how Jews interacted with Italian society.

Generally, this study focuses on the Jewish communities themselves (Shulvass estimates the Jewish population around 1600 at 35,000) with very little emphasis on the effect of Judaism on the non-



Jewish world around it and on the emerging free Jewish world in the Netherlands. Shulvass notes the impact of the Renaissance on the traditional religious communities and shows how these communities managed to retain their orthodoxy despite the influences of the Renaissance. What has become more interesting for intellectual historians of the period—the influence of Jewish, and the formerly Jewish writers and thinkers on the general Renaissance and Reformation currents—is hardly treated. He does not mention Jewish Averroism. Nor does he examine the roles of the Jewish humanists around Pico and Ficino or of Abraham de Balme in retranslating Arabic thought. Off-beat Jewish movements, such as that led by David Reubeni are likewise excluded, as are the roles of various Jewish writers like Leone Ebreo and his father, Don Isaac Abarbanel, plus some of the converts, on Messianic and Millenarian movements of the time. Finally, the polemical writings of some of the Marranos, like Elijah Montalto, that were to be so important for Amsterdam Jewry (whose chief rabbi, Saul Levi Morteira, came from Italy) are not discussed.

The author's study deals primarily, and almost exclusively, with normative Judaism and how it preserved itself in the Italian context. As such, the work is rich in detail. Those interested in how Judaism and heterodox Jewish thinkers affected the wider world and contributed to aspects of the Italian Renaissance, however, will have to look elsewhere.

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FRANCES A. YATES. *Astraea: The Imperial Theme in the Sixteenth Century*. Boston: Routledge and Kegan Paul. 1975. Pp. xvi, 233. \$21.75.

For almost three decades, Frances Yates, through her knowledge of art, music, literature, and philosophy, has shed new light on our perceptions of European ideas, especially in the Renaissance and seventeenth century.

*Astraea* fills in areas between the author's earlier books: *The French Academies of the Sixteenth Century* (1947), *The Valois Tapestries* (1959) and *The Rosicrucian Enlightenment* (1972). *Astraea* traces the symbols and ideas of the universal empire as they were revived and used for Emperor Charles V, translated and transformed for Elizabeth of England, Charles IX, Henry III, and Henry IV of France. The Roman world-ruler concepts and trappings, including eponymous Trojan founders, are imitated and altered to serve the national monarchies of France and England. In England the depictions of Elizabeth as Astraea, Virgin Justice reforming

the world, expressed an aspect of the imperial ideal. In France the elaborately planned royal processions and celebrations displayed the French monarchy as the ideal of reconciliation in times of religious civil wars. Reform and reconciliation, attributes of universal rule, are transferred to fit the national monarchies.

*Astraea*, though stimulating and rewarding, is uneven. It is a collection of items dating from 1945 through the 1950s; and in spite of the author's assertions, the parts do not always relate clearly to the whole (e.g., the chapter on the Joyeuse Magnificences). Nor does the evidence always support Yates' statements (e.g., Erasmus as a link to an Elizabethan view of Charles V as a reformer [pp. 55-6]). Though there are flaws, these essays make up a stimulating and worthwhile book.

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CHARLES TILLY, editor. *The Formation of National States in Western Europe*. (Studies in Political Development, 8.) Princeton: Princeton University Press. 1975. Pp. xiv, 711. Cloth \$22.50, paper \$4.95.

This volume represents an effort of political scientists and historians to test current theories of political development against the European experience. Supported by grants from the Ford, Rockefeller, and National Science Foundations, an international group of scholars under the leadership of Gabriel Almond and Charles Tilly participated in seminars and workshops which produced the papers presented here.

Tilly, "a sociologist who often works with historical materials," makes the major contribution to the success of the book. Historians will find themselves most at home with his introductory essay, which, in addition to providing reviews of all the papers and a summary of themes, includes some sophisticated reflections on the nature of the available evidence for the study of European state-building and on the kinds of questions which studies of this sort might be expected to answer. With respect to the pre-sixteenth-century background, Tilly stresses the cultural homogeneity of Europe, the prevalence of a peasant society, the existence of a decentralized but relatively uniform political structure, the early development of specialized organizations, the openness of the European periphery, and the growth of cities and early capitalism as factors favoring the growth of national states.

Tilly's discussion of recurrent themes points out that although his authors did not try to agree on a common definition of state, they tended to converge on a rather narrowly political notion of



"stateness." They found too that "nation-building"—the development of national consciousness, participation, and commitment—was something that generally occurred in Europe after the formation of strong states. Contributors agreed on the tremendously high costs of state-building in terms of death, suffering, loss of rights, and the forced surrender of land, goods, and labor. The largest single incentive to extraction of resources and the chief means of state coercion was the formation of standing armies.

Space allows little more than a brief listing of the other essays. Samuel Finer, a British historian, analyzes the building and maintenance of armed forces. Gabriel Ardent, a Swiss economist and historian, treats the fiscal means by which states extracted resources from their populations. Rudolph Braun, a Swiss historian, contributes a comparative study of taxation in Britain and Prussia. David Bayley, an American political scientist, studies the development of police systems. Tilly's essay deals with problems relating to food supply and public order. Wolfram Fischer and Peter Lundgreen, German historians, write about the recruitment and training of administrative and technical personnel. Finally, Stein Rokkan, a Norwegian political scientist, proposes a set of variables as a possible paradigm for research on variations within Europe. All the contributions are highly informative, but none match Tilly's for readability.

Ironically, given the sponsorship and purpose of the volume, Tilly concludes "that our ability to infer probable events and sequences in contemporary states from an informed reading of European history is close to nil." Despite the enormously different circumstances that make direct inferences nearly worthless, however, he still believes that "some general relationships among the ways of building state power, the forms of relationship between men and government, and the character of the political institutions which emerge from the process of state-building which held within the European world still hold today." Whether this hope is justified, these essays are well worth reading by anyone interested in the political history of modern Europe.

RICHARD A. LEBRUN  
*University of Manitoba*

OREST RANUM, editor. *National Consciousness, History, and Political Culture in Early-Modern Europe*. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press, 1975. Pp. x, 177. \$10.00.

This book comprises six essays on the nature of "national consciousness" in the period from the

Renaissance to the outbreak of the French Revolution in six countries that in the nineteenth century can clearly be recognized as "nation states." The authors are Felix Gilbert (Italy), William F. Church (France), Leonard Krieger (Germany), John Pocock (England), Michael Cherniavsky (Russia), and Helmut Koenigsberger (Spain). Orest Ranum's introductory essay ties together the findings of the six other historians and also explores some of the basic issues connected with the theme. Each of these seven essays is highly original and thought-provoking, and each merits a full discussion which a brief review cannot give. A few general observations must therefore suffice.

Though the essays follow different lines of approach, they seem to arrive at the same general conclusion—national consciousness was not a force to be reckoned with in the early-modern period in any of the six countries under investigation. In fact, it did not even exist, except among some English revolutionaries and, toward the end of the period, in the minds of a few continental intellectuals. It was not national consciousness that underlay the dynastic unification of the states, but rather the dynastic policies of the rulers that made possible the emergence of national consciousness in a later age. The trouble with the concept "national consciousness" lies in the adjective "national," with its anachronistic overtones of present-day "nationalism," which glorifies national popular unity and national culture, and which is quite distinct from the age-old xenophobia evident in relations between neighboring provinces and cities in the medieval and early-modern era. Though the authors do not explicitly advocate the total abandonment of the term "national consciousness" for the early-modern period, in fact each of them analyzes the changing contents of collective identities of various groups, most much smaller than the nation—a cultural elite, a dynasty, an aristocracy, or a provincial community. But a group can also lay claim to universality, as in the case of the post-Tridentine Catholic Church, the Holy Roman Emperor of the Germans, or the Christian Roman Empire of the Russians. In most instances these group identities, whether particularist or universalist, impeded rather than favored the appearance of national consciousness.

One can determine the essence of the identity of a group by analyzing its interpretation of history, its religious attitudes, and its artistic tastes. Most of the authors deal with perceptions of history by different social groups or by individuals who helped shape the outlook of the political classes. The ethos of some groups, however, may differ from, or even be antithetic to the mental disposition of the politically dominant groups. Witness

the millenarianism of the English Puritans, the lower-class antiaristocratic insistence on the purity of Christian blood in Castile, or the antisarist concept of Holy Russia. Consideration of many such frames of mind coexisting with the establishment-approved currents adds further depth to the perspectives revealed in this book.

ANDREW LOSSKY  
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PETER GAY, editor. *Eighteenth-Century Studies Presented to Arthur M. Wilson*. Hanover, N.H.: University Press of New England. 1972. Pp. viii, 197.

Arthur Wilson's many friends and admirers, including the present reviewer, will be pleased to welcome this tribute to a well-loved and highly esteemed scholar, who crowned nearly forty years of distinguished and devoted service to Dartmouth College with the completion of his remarkable *Diderot* in 1972. In view of Wilson's versatility as professor of both biography and government, this *Festschrift* is fittingly characterized by considerable diversity, for it embraces history, the history of ideas and literature. The first essay, "The Age of Personal Monarchy in England," by Stephen B. Baxter, shows that in spite of his declining role in the eighteenth century, the king could still exert considerable influence. Blake T. Hanna's study of the physicist Pierre Polinière analyzes an apparently much-used manual of the time, his *Expériences de physique* (1709), which is of interest as the work of a teacher and scientist who represents the transition from rationalism to empiricism. Paolo Casini's "Crudeli Affair: Inquisition and Reasons of State," carefully unravels the complicated threads of a fascinating little episode in Italian history and concludes that Crudeli was somewhat different from the radical thinker portrayed by Diderot in his *Entretien d'un philosophe avec la Maréchale de \*\*\** (1777). James Clifford has drawn upon his expert knowledge of Samuel Johnson to give a lively account of Johnson and his foreign visitors; he proves that the doctor's notorious hatred of foreigners in general did not exclude friendly reactions to them in particular.

Several essays deal with the philosophes. As it would certainly have been inappropriate for this volume not to contain at least one contribution by a specialist on Diderot, Jacques Proust examines the difficult question of Diderot and the legal theories of antiquity in a way that effectively opens up several avenues for further exploration. Ernest J. Knapton studies Napoleon's reactions to the philosophes, but concludes that with the exception of some extensive notes and comments on Rousseau (already discussed by F. G. Healey) and some

brief remarks on Montesquieu, the impact made by the philosophes upon Napoleon was slight. Roland Desné scrutinizes the twenty-fifth letter (the famous "Anti-Pascal") of Voltaire's *Lettres philosophiques* (1734) with a view to showing its tenuous but significant links with the English emphasis of the rest of the work. Deserving of close attention is Richard Kuhn's painstaking study, "Hume's Republic and the Universe of Newton," which examines Hume's criticism of and indebtedness to the spatiotemporal concepts of Newtonian physics.

Two thought-provoking essays deal with wider aspects of the Enlightenment. In its attempt to answer the fundamental question, "Why was the Enlightenment?" Peter Gay's stimulating contribution decisively rejects any narrow sociological explanation in terms of the influence of a single class—the bourgeoisie—and stresses the complexity of the philosophes' reactions to the world and culture of their time. Roland Mortier also answers the question his title poses—"Sensibility, Neoclassicism or Preromanticism?"—by pointing out that this question is not a matter of simple and distinct concepts but of notions which are in many ways inseparable and complementary, even when apparently opposed.

All readers interested in the eighteenth century will find something worthwhile in this volume, which is aptly introduced by a tribute to Wilson from his former colleague, John Sloan Dickey.

RONALD GRIMSLEY  
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GEORGE RUDÉ. *Debate on Europe, 1815-1850*. New York: Harper and Row. 1972. Pp. xxiii, 277. Cloth \$7.50, paper \$3.95.

George Rudé's intention is as nebulous as the title of his book. There is no "debate on Europe" except for the chapter on the Peace Settlement. Instead, he gives brief historiographical treatments of standard topics in the history of Western and, occasionally, Central Europe: Restoration, Risorgimento, liberalism, nationalism, Romanticism, socialism, the Industrial Revolution, the Revolutions of 1830 and 1848. Believing that "controversy lies at the heart of all historical inquiry," Rudé stages debates—"the successive and varying opinions of historians"—on each topic among conservative, liberal, and Marxist historians of the nineteenth and twentieth centuries. In a book of 266 pages, this often means perfunctory confrontation. In spite of his effort to define his terms, Rudé's classification of historians is not analytical but too often pigeonhole labelling. For example, Rudé calls Albert Sorel a liberal because he favors the French Revolution, but does not question the liberalism of his pro-Napoleonic stance in the pas-

sage cited. In the very issues where the labels might seem more appropriate—economic growth, urbanization, the standard-of-living controversy—Rudé admits that the labels often do not fit. This must be vexatious for the undergraduate or graduate student cramming for “Prelims,” for whom his book seems designed.

Rudé does not cover adequately monographs and substantial syntheses but relies heavily for his citations on surveys and compilations such as the Heath series. Many historians one would expect to find are omitted: the old masters, Ranke and Macaulay; more recent historians such as Ritter, Holborn, Joll, Schnabel, Krieger, and Beik; others are dismissed with one or two references: Gordon Wright, Duroselle, Heaton, Rothfels, David Thomson, Brogan, Priscilla Robertson, Landes, to mention only a few. Considerations of space are evidently not the sole reason, since E. J. Hobsbawm is referred to as an authority on all topics: he is cited nearly 50 times (not all listed in the index) in text or footnotes, including about 20 pages of laudatory exposition or quotation. Only Marx and Engels rate as much attention. An inordinate amount of space is devoted to E.P. Thompson and the Hammonds.

Whether intentionally or not, the book serves as an unproclaimed manifesto of the new orthodoxy of the “newer social history,” or “history from below” of Hobsbawm, Thompson, and Rudé himself, and he devotes roughly a third of the book to this wave of the future. One is puzzled that he mentions, only to drop, a crucial issue facing social historians: the debate of the “impressionists” like Hobsbawm versus the quantitative historians and demographers.

EVALYN A. CLARK  
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CHARLES TILLY *et al.* *The Rebellious Century, 1830-1930*. Cambridge, Mass.: Harvard University Press, 1975. Pp. xi, 354. \$15.00.

The Tillys begin this book by informing us that they have “reduced the unending richness” of all forms of collective violence to statistics: participants per 100,000 population (p. 16). Their admission provides a fitting epitaph for this fundamentally unsatisfactory book. Not only do they display an appalling disregard for the subtleties and complexities of history, but they fail even to measure up to their own standards. Thus the authors scavenge for evidence and misrepresent major developments.

Charles Tilly writes on France, Louise Tilly on Italy, and Richard Tilly on Germany. Charles’ chapter, the only one propped up by significant statistical scaffolding, reasonably argues that no explicit

deterministic relationships exist between economic phenomena such as price and wage fluctuations and the incidence of social violence. He believes that violence expresses struggles for political power. By taking that proposition to extreme lengths, however, and excluding a broad range of historical experience, he gets himself immediately into trouble. The statistics on violence that he cites either demonstrate the obvious, that 1848 witnessed more violent events than any other year and that police repression in the 1850s kept violence to a minimum, or they refute his own argument by showing that political upheaval occurred precisely at those times when social tensions reached intolerable levels (the late 1840s, 1897-1910) or when the entire nation was gripped by a general economic crisis (the 1930s). Charles also fails to take full advantage of his chosen approach. He would be less confident about the autonomy of politics were he to consider such measurable quantities as work speed-ups, the relation of wages to profits, the rate of the concentration of capital, and the subordination of independent craftsmen to capital—to suggest only a few.

The confusion that remains may result from Tilly’s refusal to permit history’s actors to intrude onto his statistical abstraction. Thus, he casually remarks that he will not discuss the Paris Commune, because his model has no room for the massive arrests that took place after the Commune was suppressed and “therefore did not enter into our statistics.” He judges that 1870-71 remains a “doubtful case in the correlation between extent of violence and extent of political change (p. 60).” One might ignore such a *gaffe* were it not indicative of the entire book’s tendency to trivialize momentous episodes of social struggle in the name of statistical neatness. Elsewhere, Charles concludes a discussion of the street battles of February 1934, with the astonishing assertion that they originated in “nonviolent” efforts to take power (p. 52). Are we to suppose that the demonstrators (Camelots du Roi? Communists?) expected that they need only huff and puff and blow the government down? Elsewhere he reproduces from Pierre Pierard some graffiti from Lille in 1868. One reads: “Nous ne voulons pas nous laissés mourir ou mangé par les Anglais” (p. 20). He does not pause to consider the curious spectacle of workers taking up the protectionist slogans of their bosses.

Neither Louise nor Richard Tilly can match their collaborator’s statistics. Hence comparisons, which the authors attempt, prove fruitless. Louise gets her Italian history mostly straight with the aid of standard secondary accounts. In her effort to homogenize very different events (a goal that all three authors strenuously pursue), however, she resorts to a sociological evasion. Faced with the

task of making sense of the special circumstances surrounding urban and rural insurrections over a century, to say nothing of the Fascist coup, she ascribes much of the violence to a pervasive "localism" that pitted regions against each other and against the national government. Her theoretical foundation for this interpretation rests on several studies of mid-twentieth-century Italian society. Irrelevant comparisons abound. Louise mentions that the incidence of collective violence in Italy does not approximate the "enormous frequencies of death, damage, and destruction" in present-day India (p. 123). Surely the peasants of the *mezzogiorno* and the laborers in the rice fields of Lombardy would have rejoiced to learn that their miseries could not compare with those of the street beggars of Calcutta.

Richard Tilly finds for Germany that "occupational groups" (labor unions) increased their political action in direct proportion to the decline of "simple crowd" violence. He explains this by reference to the disappearance of "communal" interests, which are replaced by workers' experience in productive relationships, brought on, he says, by "specialization (p. 226)." Does he mean that non-specialized (unskilled?) labor did not experience and identify exploitive relationships? Does he mean that unskilled labor vanished from the labor market by the end of the nineteenth century? He cannot mean that early capitalist production did not disrupt the local market, for he asserts (p. 201) that concentration on producers' goods in the 1840s wreaked havoc in traditional society. Richard notes the progressive replacement of soldiers by police to put down demonstrations. He says that "even as late as 1910" generals had "serious misgivings" about using soldiers to maintain order (p. 219). The significance of why, as war clouds gathered, German generals would be reluctant to order German soldiers to shoot down German civilians escapes him.

Language provides a smokescreen behind which the Tillys perform their sociometrical tricks. Charles characterizes the social struggles of the 1840s and the 1930s in France as "energy flowing into collective violence and . . . particles in which energy was emitted (p. 70)." Louise speaks of the "demobilization" (p. 93) of workers following the Fascist victory, by which she seems to mean the smashing of trade unions by Fascist *squadre* and the police. Richard uses similar language. "Modernization" possesses "significant power to influence collective violence (p. 236)." Vormärz Germany experienced "social distress (p. 208)." Crime, violence, and political radicalism may be symptoms of "social breakdown" (p. 215).

Finally, the authors assert that they are Marxists—albeit modernized. This is false on two

counts. First, they ignore the fact that Marx's theoretical and historical analyses turn on the dynamics of class relationships. Only within that framework do political struggles of this kind make sense. The Tillys substitute the meaningless notion of "solidary groups" [*sic*] for the concept of class. Marx, they tell us, emphasized the impact of industrialization on the political process. Actually, he did not. He emphasized the progressive development of capitalist social production and the political stresses that accompanied it. That is clear in a passage from *The Class Struggles in France* that the Tillys themselves quote to prove the opposite of what it says (p. 272). Second, they make Marx an accomplice in sentimentalism and spurious radicalism. "Our problem today," they write, "is to transform a single world of war and injustice into a single world of peace and justice (p. 299)." Such was, they suggest, Marx's vision. Perhaps—but he would have been quick to recognize that he and they did not belong on the same side of the barricades.

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*La guerre en Méditerranée, 1939-1945: Actes du Colloque International tenu à Paris du 8 au 11 avril 1969.* (Comité d'Histoire de la 2<sup>e</sup> Guerre Mondiale.) Paris: Éditions du Centre National de la Recherche Scientifique. 1971. Pp. 792. 96.80 fr.

The meetings in Paris, of which this book is the verbatim record, convened more than 160 persons from more than 20 countries under the auspices of the international Comité d'Histoire de la Deuxième Guerre Mondiale. Forty-eight previously prepared papers and critiques of papers were read. They were interspersed with responses to the critiques and were followed by one session devoted to general discussion. A sedulous effort to maintain scholarly objectivity and an international approach was in the main successful. The four sessions with central themes dealt with general problems of policy and strategy, conduct of war and military operations, international relations, and nationalism in northern Africa and the Levant. The bits and pieces, when combined in one book, provide both substance and guidance to further investigation.

Most aspects of the Second World War in the Mediterranean region are either mentioned or more fully treated: the divergences in the war aims of the members of each coalition, the erosion of the original Axis concept of two parallel wars, Hitler's need of French collaboration in view of British obduracy, the usefulness to both sides of a neutral Spain, Turkey's tightrope act as a neutral, the consequences of changes in the membership of

each coalition as the Soviet Union was expelled from its association with the Axis powers and as Italy was pulled away from its ties with Nazi Germany, the value of the contribution to victory made by the Mediterranean campaigns in view of Soviet successes elsewhere, the failure to subdue Malta, logistic support of the German-Italian *Panzer Armee Afrika*, the political motivation for operations in southeastern Europe undertaken or proposed by the British, the impact of French defeat, division, and dependence on others for modern arms upon Anglo-American relations with French leaders, Stalin's role in the decision to invade France from the south as well as the west, contributions by organized Resistance units to Allied successes in southern France and northern Italy. The tactics of campaigns claim little notice, however, except for a succinct account by Colonel Le Goyet, a French Army historian, of the outstanding performance by French forces in Italy.

Certain participants in the colloquy were already well-known specialists in the history of World War II; others, less prominent, also dealt effectively with primary matters of fact and interpretation. A few persons were able to provide data from their first-hand, wartime experiences. One was René Massigli, the diplomat. Others were French officers involved in the French Forces of the Interior and Italians who had opposed the Fascisti before the armistice of Cassibile and the Germans until the war ended. While more than half of those attending were French, historians came from communist countries of eastern Europe, from Israel, Turkey, Greece, Austria, both Germanys, the Low Countries, Italy, the United Kingdom, Switzerland, and the United States.

The significance of the colloquy is dual; it is a stride toward a sound history of the war; it is also an episode in the history of history.

GEORGE F. HOWE  
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DONALD CAMERON WATT. *Too Serious a Business: European Armed Forces and the Approach to the Second World War*. Berkeley and Los Angeles: University of California Press, 1975. Pp. 200. \$8.50.

Georges Clemenceau's dictum that "war is too serious a business to be left to soldiers" provides the title and one theme for this ambitious study of the background to World War Two. Donald Cameron Watt examines the way military leaders in Britain, France, Germany, and Italy carried out their multiple assignments in the interwar period. He concludes that they all failed to some extent to come to terms with the new technology of war and produce realistic strategic doctrines. This failure, he argues, stemmed from the structural defects

inherent in the internal divisions of their national societies. Military men confronted the prospect of war in 1939 with a gloomy pessimism unlike the prevailing attitude of 1914. When the war came in spite of their warnings, it was—until 1941, at least—a civil war which reflected the breakdown within Europe.

This book originated as a series of lectures at Cambridge University in 1973. It is brief and suggestive rather than conclusive at many points. In his use of the concept of European civil war, for instance, or in supporting his contention that "despair or at least despondency to the point of defeatism" was common to the General Staffs of possibly three of the four powers at war in 1939, Watt will leave some readers anxious for more evidence. He succeeds brilliantly, however, in synthesizing a wide range of secondary and primary sources and presenting an original and useful interpretation of the origins of the Second World War. This book should stimulate scholars to move away from traditional diplomatic and military history into a broader consideration of how strategy relates to politics and where the European system failed.

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LIVIA ROTHKIRCHEN, editor. *Yad Vashem Studies on the European Jewish Catastrophe and Resistance*. (Yad Vashem Studies, 10.) Jerusalem: Yad Vashem; distrib. by Ktav Publishing House, New York, 1974. Pp. 326. \$15.00.

This is the tenth volume, besides its bibliographic series and documentary texts, that Yad Vashem, the Martyrs' and Heroes' Remembrance Authority established by the Israeli government, has published since 1957. It deals primarily with the Holocaust, the term now widely used to describe the destruction of the Jews by the Germans during the Second World War. The steady accumulation of Yad Vashem's studies and their improved caliber, especially under Livia Rothkirchen's editorship, have produced a useful, if undistinguished, corpus of historical papers on the Holocaust.

Three papers in the current volume are devoted to the problems and politics of Jewish leaders under German occupation in Holland (by Joseph Michman), in Hungary (by Randolph L. Braham), and in Germany 1933-38 (by Abraham Margaliot). Based mainly on unpublished sources, these three papers provide some new and interesting data, but they fail to provide guidelines or establish criteria for using evidence of the involved parties. Since the personal bias and political partisanship of eyewitnesses and survivors customarily



warp the historical record, the historian is obliged to treat these sources critically and, if possible, rectify the distortions.

Leni Yahil, author of the splendid *Rescue of Danish Jewry*, has brought together eleven selected documents from the British Foreign Office and Cabinet concerning the illegal immigration to Palestine 1939–40. Her historical introduction is marred, however, by ideological bias.

Martin and Eva Kolinsky, in "The Treatment of the Holocaust in West German Textbooks," examine twenty-five elementary and secondary textbooks of postwar Germany in light of the country's educational policy and objectives. The authors submit a variety of topics (National Socialism, the Nazis, anti-Jewish policies and programs, the destruction of the Jews, German concepts of Jews, German attitudes toward war crimes) to close critical analysis and subtle textual explication. They conclude that, with the notable exception of Hannah Vogt's *Schuld oder Verhängnis?* (published in English as *The Burden of Guilt*), the textbook treatment of the Holocaust in West Germany is characterized by neglect and evasion.

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GEORGE CLINCH. *English Costume: From Prehistoric Times to the Eighteenth Century*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xxii, 295, 131 illustrations. \$14.50.

G. T. SALUSBURY-JONES. *Street Life in Medieval England*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 213. \$12.75.

Here are two examples of old-fashioned social history: no four-cell tables, no chi-square correlations, no attempt to relate the subject to social and institutional structure. These reprints have a mean age of 51 years (though Salusbury-Jones did a 1948 re-edition wherein he drew some parallels between life in the years of World War II and the street-life milieu of the later Middle Ages). Both are pleasant and informative books from a long time ago. If neither reflects great analysis or deep conceptualization, they do show a sympathetic eye for some basic if prosaic aspects of preindustrial life.

Clinch's book is more a work of reference than a narrative. He moves chronologically, in short chapters, from prehistoric costume to that of the eighteenth century, and then concludes with chapters on such topics as the garb of medieval times (with items of clothing listed alphabetically), of the church, of the orders of chivalry, etc. This book, with its many illustrations, is an antiquarian handbook that takes its place beside similar works

of the day on church furnishings, monumental brasses, and academic and legal costume. With brasses, effigies, and illuminations as his main sources, Clinch describes the outer garments (though not the underwear) of various social classes through the centuries. He ignores the social and sexual aspects of fashion, the economics of cloth and the clothtrade, and the technological side of the garment industry. He offers a useful handbook to deal with that basic daily question, "What should I wear today?" as asked by millions of people.

Salusbury-Jones' book consists of six essays on various facets of urban social life. Traffic congestion, the hours and enforcement of the curfew, and the lack of bequests to build public latrines are the sort of things he discusses. His main sources are printed municipal and borough records such as the *Calendars of the London Letter Books* and the *Coventry Leet Book*. His social history is of the genre of Salzman's *English Life in the Middle Ages* (1926) and Jusserand's *Wayfaring Life* (3rd edition, 1925). The author conveniently puts together things we would have to dig around for on our own. He tends to take the records at face value, but like Clinch, he offers ready information we do not get, let alone remember, from reading scholarly monographs.

Neither book was reviewed in the *AHR* upon first appearance. Their belated recognition symbolizes an attempt to bridge the gap between books used by theatrical costume- and set-designers and historical novelists, on the one hand, and professional teachers on the other. To still be amusing and informative, and for a wide audience at that, is not a bad fate for a couple of old horses.

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GLANMOR WILLIAMS. *Glamorgan County History*. Volume IV, *Early Modern Glamorgan: From the Act of Union to the Industrial Revolution*. Cardiff: Glamorgan County History Trust Limited; distrib. by University of Wales Press, Cardiff. 1974. Pp. xviii, 717. £15.

*Early Modern Glamorgan*, the third volume of the *Glamorgan County History* to appear, carries the account forward from the Act of Union of 1536 to the beginning of the Industrial Revolution. Its ten chapters—backed by genealogical tables, maps, and a wealth of excellent illustrations—trace the economic and social life of Glamorgan; its political, religious, and literary history; and aspects of education in the county during the period 1660–c.1775.

In many ways the early modern period was a golden age in Glamorgan history. Union with Eng-



land marked the end of the turmoil of the Middle Ages and inaugurated an era of growth and prosperity during which many of Glamorgan's leaders ventured with notable success onto the wider stage of English political life. Historians of English politics are already familiar with the achievements of Sir Leoline Jenkins and successive generations of the Herbert family; they may be surprised to discover that these were only part of a galaxy of talented individuals who helped to shape Glamorgan society during this period.

In a corporate work of this nature it is invidious to single out individual contributions. But Penry Williams' essay on the political and administrative history of Glamorgan between 1536 and 1642 deserves to stand as a model for this genre. A masterpiece of clarity and compression, its value is enhanced by the existence of parallel accounts, notably for Wiltshire in the *Victoria History* of that county.

Indeed, it is with the "general" volumes of the *Victoria History* series that this venture seems most likely to be compared. In format they are similar. But freed from inhibiting traditions, this volume of the *Glamorgan History* has achieved a cohesion and clarity which occasionally escapes its English counterpart. Whether multi-volume county histories have become an expensive and expendable luxury is a perennial question. But, at £15 (\$30) for more than 700 excellent pages, *Early Modern Glamorgan* hardly constitutes an overwhelming case for the abolitionists.

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R. W. DUNNING, editor. *A History of the County of Somerset*. Volume 3. (The *Victoria History* of the Counties of England.) New York: Oxford University Press, for the Institute of Historical Research. 1974. Pp. xx, 293. \$77.00.

The appearance of a new volume of the *Victoria History* of the Counties of England for a certain "unmentionable county" in the West of England is an event so rare as to fire the highest expectation in a historian of Somerset. This volume, the third, comes almost two-thirds of a century after the last (vol. 2, 1911), which followed the first volume (1906) by only a few years. The first two volumes constituted the "general" history of the county; the present volume is the first of a series of "topographical" histories of individual hundreds, parishes, and boroughs. This volume, really an entirely new endeavor as far as Somerset is concerned, lives up to expectation. It maintains the highest standard of scholarship, the aim of the original editors of VCH, with the current broad-

ening of concerns and wholehearted acceptance of new facets and methodologies in history that the present general editor, Dr. R. B. Pugh, has brought to the project since assuming its direction in 1949. Perhaps the contrast between the older, "Victorian" scholarship of VCH and the newer "Elizabethan" scholarship of our day is made more vivid by the long lapse of time between this volume and its predecessors. But the contrast is heightened by the meticulous modern scholarship of the editor of volume 3, R. W. Dunning, and his assistant, R. J. E. Bush, two young scholars who are almost entirely responsible for the text. They have executed a remarkable achievement: a comprehensive, chronologically even, accurate, multifaceted, and—perhaps incredible in such a compendium—readable history of four south-central hundreds of the county, Kingsbury East, Pitney, Somerton, and Tintinhull. Pursuant to the new canons of VCH, each parish or town within a hundred is treated uniformly in terms of manors and landholding, economic developments, local government, ecclesiastical history (established church and Nonconformity), education, and charities, with attention paid to historic buildings. The chronological range is truly from prehistory to the present.

Perhaps the principal criticism that can be leveled at the volume is in fact one aimed at the whole endeavor. Somerset desperately needs a new, modern brace of "general" history volumes—those of 1906 and 1911 are hopelessly out of date. The enormous cost of producing—and, at £24, purchasing—a volume of so limited geographic scope is such as to put in question the entire concept of VCH in the 1970s. Had the original objective of writing all the histories of all the counties been accomplished within a few years of the demise of the regal patron, VCH would have been an eternally useful, albeit increasingly dated, recueil for the study of local history. Now the disparity between the new and the old is such as to rob all of an essential unity. One remarks ruefully that the *Victoria History* of the Counties of England has not only outlived Victoria but also the ancient counties. Somerset, along with other ancient counties, has just been chopped out of recognition in the last Tory government's scheme of reorganization of local government. VCH has been fated "to hang / Quite out of fashion, like a rusty mail, / In monumental mockery."

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WILLIAM J. BARBER. *British Economic Thought and India, 1600-1858: A Study in the History of Develop-*

*ment Economics*. New York: Clarendon Press of Oxford University Press. 1975. Pp. viii, 243. \$19.25.

This book studies the relationship between the activities of the British East India Company from its founding until the Sepoy Mutiny and the state of British economic thinking. Its contributions lie in the area of the history of economic analysis, and some knowledge of economic theory is needed by the reader if he is to profit from the book.

Since British policy in India was of considerable economic importance and the subject of a great deal of political interest, it is scarcely surprising that most outstanding British economists of the period between 1600 and 1858 became involved with Indian problems. Various economists were Company administrators, apologists, or critics. They sometimes served in more than one of these roles. The list of such economists discussed by Barber is an impressive one: Thomas Mun, Sir James Steuart, Adam Smith, Lord Lauderdale, James and John Stuart Mill, Thomas Malthus, J. R. McCulloch, and Karl Marx. Naturally, the level of intellectual involvement with India was not equally great, but Mun, Steuart, Lauderdale, James Mill, Malthus, and McCulloch all devoted major amounts of time and effort to Indian problems.

Three of the episodes discussed in the book are particularly interesting and informative. The first is Thomas Mun's defense of silver export within the intellectual context of mercantilism. His case rests on specie's being a factor of production in international trade activities.

The second matter concerns the anonymous pamphlet *Considerations Upon the East-India Trade* published in 1701. This remarkable work was a precursor of the doctrines of the classical economists in its analysis of competitive markets. Barber makes a convincing case that this apparently precocious work was really a logical result of the policy discussions then in progress.

The third section concerns the dispute between James Mill and J. R. McCulloch as to which aspects of Ricardo's doctrines should be applied to the governance of India. Mill was obsessed with the theory of differential rent and its implications for the theory of land taxation, while McCulloch stressed the benefits of free markets and the absence of governmental interference.

The book is well written and should be of considerable interest to students of the history of economic analysis.

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CHRISTOPHER HAIGH. *Reformation and Resistance in Tudor Lancashire*. New York: Cambridge University Press. 1975. Pp. xiii, 377. \$23.50.

Lancashire was the most Catholic county in Elizabethan England, but the factors causing this phenomenon have never been fully explored and closely examined. In his well-documented monograph, based largely upon manuscript sources, Christopher Haigh puts forward a feasible explanation. Lancashire was remote, isolated, and backward, especially when compared with southern counties, and its religious history was unique. Medieval heresies may have thrived elsewhere, as Dickens has demonstrated, but not in Lancashire. The unwieldy ecclesiastical structure—with its divided jurisdiction, its exceptionally large parishes, and its unremunerative benefices—was not congenial to either Erasmian or Cromwellian reforms. In fact, both the clergy and the laity resisted the Henrician Reformation, particularly the dissolution of the monasteries. The Edwardian reforms were well received in the southeastern parishes—those closest to Oxford, London, and the outside world—but they were not enforced in the rest of the country. Thus the old faith, revitalized during the reign of Mary, survived almost intact. The Elizabethan Settlement, however, polarized Lancashire society. Attempts to enforce the established religion and eliminate old practices gave rise to recusancy, crypto-Catholics, secret masses, Jesuit missions, flights to Ireland and Douai, and appeals to Rome. These, in turn, evoked a Puritan reaction characterized by sermonizing and witch-hunts. The Established Church, caught in these crosscurrents, proved to be weak and powerless in the struggle. Thus did cohesiveness of an isolated society give way to competition, and Lancashire became the "cockpit of conscience."

Except for repetitious restatements of the thesis, the work is solid and well written. The author has included relevant statistical material and comparisons with parallel studies. A map and a definitive bibliography enhance the value of the book.

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L. M. HILL, editor. *The Ancient State Authorities, and Proceedings of the Court of Requests by Sir Julius Caesar*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1975. Pp. xlvii, 281. \$25.00.

The rivalry between the conciliar or prerogative courts and the common-law courts in the late sixteenth and early seventeenth centuries was once thought to be a conflict between "English" common law and "Roman" civil law. W. J. Jones in his work on Elizabethan Chancery demonstrated that this was not so. Rivalry there was, but the cause was more mundane than ideological. Two separate, but not totally distinct, judicial systems competed for fees, fines, influence, and jobs. Long

before Star Chamber and Chancery felt the full force of the common lawyer's assault, the "Poor Man's" Court of Requests came under challenge. Sir Julius Caesar, a Master of Requests and an Admiralty judge, rose to its defense with the publication of his *Ancient State Authoritie*, which is now reprinted for the first time.

Caesar's book serves today as one of our principal sources for the work of the Court and demonstrates the pragmatic nature of the contest between the rival systems. In 1591 Caesar was appointed a Master of Requests, and the court's jurisdiction was challenged in Locke vs. Parsons in Common Pleas. Caesar immediately began his compilation of *The Ancient State Authoritie*, using the same methods in defense of Requests that his common-law opponents used in defense of their system—the historical record. Locke and other common lawyers might challenge the *de jure* existence of Requests, but Caesar proved its *de facto* existence. He also asserted that to attack Requests was to attack the jurisdiction of the Council itself—an attack no one was yet ready to make.

L. M. Hill has provided a useful introduction to the book and to the life and career of Caesar. He is honest enough not to inflate the importance of Caesar's work, admitting that the usefulness of the court did more to prolong its life than did the publication of Caesar's defense.

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CLIVE HOLMES. *The Eastern Association in the English Civil War*. New York: Cambridge University Press. 1974. Pp. x, 322. \$17.50.

This excellent book illustrates and explains much more than local history. It establishes several important correctives to the older accounts. Far from exhibiting an ideological homogeneity, the Eastern counties manifested various shades of political and religious opinion on the eve of war. The achievement of unity and commitment required strenuous political activity, both in the counties and at Westminster. Holmes focuses on the activities of the committee of the associated counties of Suffolk, Norfolk, Essex, Cambridge, Lincoln, Hertford, and Huntingdon for the years from 1642 to 1645. Expert analysis of a rich body of records, manuscript as well as printed, elucidates the problem of getting the counties to act together and sheds further light on personnel of local administration and the fiscal innovations of the war. In respect to the last, Holmes has provided the fullest account of the monthly assessment, the most important of the tax measures devised during the revolution. Although enormous sums were raised, a recurrent litany of

fiscal despair runs through the official correspondence, balanced by complaints from Manchester's soldiers regarding their arrears of pay. Holmes gives much attention to the recruiting of the army, modifies often-quoted descriptions of the Eastern Association's cavalry as composed primarily of men of substance, and proves the strong ingredient of tradesmen and apprentices. He explains Cromwell's quarrel with Manchester much more fully than anyone has done and notes the importance of the quarrel in the disputes between the parties in Parliament. On the whole, no other book gives so good a picture of the place of localism in the Civil War.

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C. D. CHANDAMAN. *The English Public Revenue, 1660–1688*. New York: Oxford University Press. 1975. Pp. xii, 386.

This is an important, but flawed book. The tables which provide detailed information about government revenue, issues, and assignments from 1660 to 1688 will replace the incomplete, misleading, and sometimes inaccurate figures presented by the late William A. Shaw in his introductions to several of the first eight volumes of the *Calendar of Treasury Books* (London, 1904–23). This new set of accounts is accompanied in the extensive appendices by an elaborate explanation of the information which can be extracted accurately from the Exchequer accounts, but readers must understand the explanation before using Chandaman's tables.

The administrative history of the English public revenue is explained in chapters concerning the customs, excise, hearth money, direct taxes, and numerous small and casual branches of revenue. While this book does not supersede the works of E. Hughes and C. A. F. Meekings on various aspects of revenue administration, it is a comprehensive account containing a great deal of previously unpublished information. However, there are errors. A regrettable lapse which might cause the unwary some inconvenience is the use of the Public Record Office classification GD when citing the important Leeds and Shaftesbury manuscripts, a classification which was changed to PRO 30 almost fifteen years ago. Chandaman's interpretation of the political and constitutional ramifications of government revenue is not entirely convincing, and more sophisticated statistical and analytical techniques would have made his arguments more persuasive. But with these limitations in mind, the book must be valued for the important contribution it makes to our knowledge of government revenue and administration. If the book's price does not prevent its purchase, it will serve as the new starting point for further research

in this important area of seventeenth-century English history.

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LOIS G. SCHWOERER. "No Standing Armies!" *The Antiarmy Ideology in Seventeenth-Century England*. Baltimore: Johns Hopkins University Press. 1974. Pp. x, 210. \$10.00.

Armies, however useful in defending societies, habitually pose a threat to their own civil governments. This fact has been recognized since the earliest years of the modern state, when rulers first began trying to create permanent armies, and representative assemblies stoutly resisted them as a menace to public liberties and an unwarranted burden on national treasuries. In England, geography afforded sufficient security against foreign dangers to give the antiarmy argument unusual weight, and a taste of military rule under Cromwell was equally persuasive. When the peacetime standing army first appeared in England on a regular basis after 1660, therefore, it had to do so furtively, like the Cheshire cat, and it was not until the century's end that it was grudgingly accepted, under parliamentary auspices. The struggle left its marks, and the special character of the Anglo-American tradition of civil-military relations, combining a deeply rooted skepticism about the army with a determination to keep it limited in size and clearly subordinate to the civil power, had been firmly established.

Lois Schwoerer's monograph examines the body of pamphlet literature and parliamentary debate about the creation and control of the standing army and the regulation of the militia in the crucial period after 1628. She shows how long-lived the seventeenth-century arguments were, frequently plagiarized well into the following century, but suggests that after 1660 antiarmy prejudice was often used in partisan fashion merely as a convenient stick to beat ministries.

Doubtless this work will be useful primarily to specialists in later Stuart politics where its main strengths lie, but it is also a piece of the important but long neglected enquiry into the origins of the liberal tradition of civil-military relations. The relevance of that larger study to our own age ought to be clear.

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EDWARD B. POWLEY. *The Naval Side of King William's War, 16th/26th November 1688-14th June 1690*. Fore-

word by SIR ARTHUR BRYANT. [Hamden, Conn.: Archon Books. 1972. Pp. 392. \$11.75.

This book should be entitled *William III's Navy and the Struggle in Ireland*, because that describes its main contribution, but it is not easy to construct a title for a book so disjointed and amorphous. The work is ostensibly a detailed naval history from November 1688 to June 14, 1690, a fortnight before Beachy Head. That battle was to the English navy what Pearl Harbor was to ours, and England has probably never been so close to successful invasion in modern times. Yet Powley's treatment of its antecedents is, when compared with the rest of the book, hurried and sketchy—seemingly unfinished. John Ehrman's chapter in *The Navy in the War of William III* remains the best account, though we still do not know the precise role of financial turmoil, victualling collapse, and sickness among the seamen in the mobilization effort of 1690.

The book is not, as Sir Arthur Bryant asserts in a foreword, "a work of scholarship of the highest quality"; rather it might be compared with an ordinary Ph.D. dissertation—impressive in research and detail, but atrociously conceived and written. The organizational scheme resembles that of the Anglo-Saxon Chronicle; the style wanders between archaism and modern colloquialism; and the misleading syntax offers challenges to experts and laymen alike. The fact that the author died before the book was prepared for the press may account for some of the flaws. Nevertheless, since its only value lies in the richly detailed information (especially on Irish campaigns) that it provides for specialists, from not only obvious materials but also ships' logs, French archives, and other recon-dite sources, a microfilmed version would have served well enough.

DANIEL A. BAUGH  
Cornell University

HARRY CARTER. *A History of the Oxford University Press*. Volume 1, *To the Year 1780*. With an appendix listing the titles of books printed there, 1690-1780. New York: Oxford University Press. 1975. Pp. xxxi, 640. \$48.00.

Georges Clemenceau's view that war is "too serious a business to be left to soldiers" is now a commonplace, often applied to other, less bellicose activities. Thus readers may be surprised to discover that this impressive history was written not by a historian but by "one who is primarily a printer." Whether they will be disappointed depends on their expectations. Because the history of any distinguished press is inextricably linked to intellectual history, it cannot properly be confined to administration, as Harry Carter fully appreciates. The present volume, intended to be the first

of two or three, stands on the archives of the press, university, and Public Record Office and on other diverse sources of information. The approach, informal in tone, is bibliographical, in part because of the author's competence and in part because a collaborative history of the university is now in progress.

Ten introductory chapters describe the character and career of presses at Oxford up to 1690, the mounting emphasis on the need for a university press (no German or French university was without one), and the great influence, first of Laud, then of John Fell in this direction. Fell actually established a press for the printing of learned books, of which Carter supplies brief descriptions, including typefaces and other technical matters. Four years after Fell's death in 1686 his press became in effect the Oxford University Press, as he had provided in his will. During the next sixty-five years the Press had an uneven history, owing to incompetent administration. Happily in 1755 the half-century of "unsalutary neglect" ended with the appointment of Blackstone as delegate. Thenceforth, in every respect—technical, administrative, and intellectual—the Press fulfilled the desires of Laud and Fell.

Rounding out his narrative, Carter has appended a two-hundred page list of titles printed at the Press, 1690–1780, English Bibles, Testaments, and Books of Common Prayer not included. Here one may trace, in part, the intellectual history of eighteenth-century England. From the outset Fell's ideal of printing only learned books was eroded by the appearance of pieces of the moment, the best known being Clarendon's *History of the Rebellion* (1702). Throughout the entire period Latin texts had a large place, though the source might be Greek or Arabic. The first year saw a Euclid, Suetonius, and Xenophon, the latter outshining any other historian. As might be expected, sermons, homilies, and university proceedings abounded. History and geography got persistent attention, as did grammar and, increasingly, science. The threat of Dissent was always manifest. Though by no means conforming to a pattern, the proportion of English titles steadily increased. The number of titles varied considerably from year to year—seven in 1724 and 1779 and thirty in 1755, with an average of about fifteen.

The volume is enhanced by informing illustrations, and this reviewer hopes that its successors will soon follow.

CHARLES F. MULLETT  
University of Missouri,  
Columbia

JULIAN GWYN. *The Enterprising Admiral: The Personal Fortune of Admiral Sir Peter Warren*. Montreal:

McGill-Queen's University Press. 1974. Pp. xvi, 292. \$16.00 Canadian.

This is a study of the opportunities that war presented to one Royal Navy officer for making himself wealthy. The officer was Peter Warren, third son of an Irish Jacobite, who saw his best prospects in joining the navy as an ordinary seaman in 1716. Through luck and patronage he rose in eleven years to the rank of captain. Through luck and a good marriage he acquired social status and business contacts in colonial New York. But it was war that made Peter Warren.

The war, King George's War in the English colonies, gave Warren the rank of Vice Admiral and wealth commensurate. He attained both largely through taking numerous "prizes-of-war"—French merchant ships and naval vessels in the waters of the West Indies, off the coasts of North America, and especially at Louisbourg, Nova Scotia, when that fortress fell to forces under his command in 1745. Fame and status compounded fortune, and his family, deprived of its father by Warren's early death in 1752, estimated its wealth at about £250,000 sterling by the end of the century. Indeed, "the war had made Warren a man rich in his own right" (p. 15). But the story is what he did with his money.

Julian Gwyn's book establishes in detail the origin and development of each of the several components of the Warren family fortune. We learn of Peter Warren's buying and selling of land in New York, Ireland, and England; of his monies lent out and repaid with interest in the New World and the Old; and of his investments in the London securities market. Gwyn is convincing in showing Warren to have been "a keen businessman" (p. 202), although "the impression [of Warren] . . . as an international financier" (p. 199) is clearer to the eyes of his biographer than it is to this reader. What Warren did do was to recognize and take advantage of good investments in several different places. War provided him the means by giving him the money; Warren's own talent as wise investor helped him turn a windfall into a fortune. "Enterprising" is the key word.

JOHN J. MCCUSKER  
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College Park

ROGER ANSTAY. *The Atlantic Slave Trade and British Abolition, 1760–1810*. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xxiv, 456. \$22.50.

The publication in 1944 of Eric Williams' *Capitalism and Slavery* dramatically disturbed accepted notions about the operation of the trade in African slaves by Europeans and the process by which this trade was abolished during the nineteenth cen-



tury. Although Williams was not the first to point out the relationship between slavery and the international economy—C. L. R. James had made the same essential argument in *The Black Jacobins* in 1938—his provocative style, his slightly polemical posture, and his timing all contributed to establish his work as a point of departure for new thinking on an old subject. Williams' work, after all, coincided with two seminal publications in American historiography: Gunnar Myrdal, *An American Dilemma* (1944), and Frank Tannenbaum, *Slave and Citizen* (1946). Williams' attack on British altruism—conventionally called humanitarianism—was revolutionary and invited strong counterattack. Most authors, however, were more interested in attacking the author or in refuting his thesis that abolition was essentially an economic gesture, than in trying to understand the subject. In this remarkably thoughtful and indefatigably well-researched book Roger Anstey has given a thorough examination of the history of a vital part of the British slave trade, reviewed the process of abolition, and substantially demolished the foundation of the Williams thesis.

The work is divided into four related sections. Part One deals with the slave trade and its profitability between 1761 and 1810, as well as the possible impact on some Atlantic African societies. Part Two examines the general intellectual milieu of the eighteenth century, with particular reference to theological, reformist, and anti-slavery thought. Part Three focuses on English evangelicalism, relating its substance, evolution, associations, and relationship to Quaker activity on both sides of the Atlantic. Part Four meticulously analyzes the parliamentary process of abolition, with considerable detail on the political sophistication of those who espoused the cause of anti-slavery.

The most memorable, and perhaps the most significant section is that dealing with the economics of the slave trade. Anstey considerably expands his earlier published salvos attacking the notion that the slave trade was an enormously profitable enterprise. He concludes that the average annual return for British slavers was about 10 percent, for French slavers about 7 percent, and about 2 percent for Dutch slavers. This relatively slim and highly inconsistent profit margin, he believes, was totally inadequate to foster significant industrial growth. Indeed, the estimated annual profit of the trade—a gross of about £200,000—would have contributed only about one-eleventh of one percent of the total national industrial investment required to launch and perpetuate the Industrial Revolution. Nevertheless, he concedes that in specific industries, such as cotton manufacture, and in specific localities such as the Liverpool hinterland, the impact of slave-trade profits could have been "meaningful."

Anstey does not attempt to calculate the social cost of slavery to the participating African communities. Instead, he limits his investigation to regional economics and demography. He finds that larger political groupings tended to benefit at the expense of smaller, segmented societies, but that several factors contributed to economic success in any particular region. The operation of the trade was a complicated business for both European and African merchant. The cost in manpower was probably negligible in the Senegal-Biafra slave-trading zone, but considerably higher in the Congo-Angola region.

The politics of abolition were, according to Anstey, fundamentally caught up in the changing ideological and theological attitudes of the later eighteenth century. The most essential aspect of this was the apparent incompatibility of slavery and liberty and the fusion of ideology and theology which lay at the root of Evangelical success. Nowhere else is the political astuteness of Quakers and abolitionists treated more clearly and more succinctly than in this work. Anstey argues convincingly that the abolitionists initiated political lobbying and exploited every opportunity from the war with France to the death of Fox to infuse some morality to national action. And by subtly separating abolition of the slave trade from a general acceptance of institutional reform, they ironically opened the floodgates for a century of reform in British institutions and British society.

Some aspects of Anstey's work are debatable. Terms such as "triangular trade" and "Negro" (as a synonym for African) will undoubtedly remain controversial. But overall this is a stimulating complement to the variety of new works, some of which are themselves brilliant, which have dealt with the Atlantic World.

FRANKLIN W. KNIGHT  
Johns Hopkins University

M. L. CLARKE. *Paley: Evidences for the Man*. [Toronto:] University of Toronto Press. 1974. Pp. viii, 161. \$10.00.

In this unpretentious monograph, M. L. Clarke examines the life and writings of the eighteenth-century theologian and moralist, William Paley, archdeacon of Carlisle and, in his own time, an author of enormous reputation. Clarke devotes the first half of his study to Paley's rather uneventful rise to fame, from his early days in Yorkshire, through his successful career at Cambridge, to his final years as a comfortably affluent member of the Established Church. Paley excelled as an undergraduate at Christ's College and became one of the university's most popular teachers. His natural informality and bucolic wit charmed his students, and his lectures revealed the same admirable lu-



city and penetrating common sense that characterized his later efforts as a writer. Clarke spends the last half of his study recapitulating these now almost forgotten works. In his *Principles of Moral and Political Philosophy*, Paley advocated a policy of utilitarianism that was more religious and less radical than that of his contemporary, Bentham. In his theological works, Paley distilled the ideas of his eighteenth-century predecessors into a system of natural and revealed religion that earned him wide respect among his Christian readers. Indeed, although the Darwinian revolution would eventually sweep away Paley's mechanistic view of nature embodied in his *Natural Theology*, the *Evidences of Christianity* would be an official part of the Cambridge curriculum until 1920.

Clarke's biographical chapters draw upon an impressive range of sources and, through a generous use of anecdote, convey the engaging personality of this neglected divine. Regrettably, the chapters on Paley's thought prove less valuable. Clarke rarely asks interesting questions and, as a result, never really grasps the underlying coherence of Paley's works or explores satisfactorily the complex intellectual traditions which shaped his thinking.

D. L. IEMAHEU  
Lake Forest College

MALCOLM I. THOMIS. *The Town Labourer and the Industrial Revolution*. (Studies in Economic and Social History.) New York: Barnes and Noble. 1975. Pp. 247. \$11.00.

*The Town Labourer* by John and Barbara Hammond was almost completely written before the First World War. Although there is a whole library of later literature on the subject of the early Industrial Revolution in England, the Hammonds' book remains in print and on reading lists for courses in British history and modern European economic and social history because it is the classic statement of the opinion that industrial change was a traumatic experience for the workers. It is this view that Malcolm I. Thomis seeks to correct.

Some sophisticated students might consider Thomis' efforts unnecessary after the achievements of Sir John Clapham, T. S. Ashton, and R. M. Hartwell. There is very little original that this book (researched mostly in secondary sources) can offer by way of a contribution to the "standard of living" debate. The author is judicious, however, and the evidence which he presents is interesting. The book might have been even better if Thomis had not focused so much on the Hammonds to the extent of quoting passages from *The Town Labourer* as chapter mottoes; much of his argument is obvious in view of the contributions of several generations of researchers.

This book is a useful addition to debate on a survey-course level, but scholars will find the arguments familiar. Since the appeal of the Hammonds has always been to the readers' emotions it is questionable whether the reasonable weighing of evidence by Thomis will change the opinions of those undergraduates whose minds on this subject were made up in secondary school. It is hoped that Thomis' book will be widely read in teacher-training institutions on both sides of the Atlantic.

JOHN W. OSBORNE  
Rutgers University,  
New Brunswick

KEITH BURGESS. *The Origins of British Industrial Relations: The Nineteenth-Century Experience*. (Croom Helm Social History Series.) Totowa, N. J.: Rowman and Littlefield. 1975. Pp. xiii, 331. \$19.50.

ARTHUR J. TAYLOR, edited and with an introduction by. *The Standard of Living in Britain in the Industrial Revolution*. (Debates in Economic History.) London: Methuen and Company; distrib. by Barnes and Noble Books, New York. 1975. Pp. lv, 216. Cloth \$18.50, paper \$10.00.

These volumes are testimony to the persistence in the profession of the narrow conceptual frameworks of previous generations. Ironically, Keith Burgess claims as "the most important justification" of his study "the desire if not obsession in every generation of historical writing [*sic*] to reinterpret the past in a way that seems relevant to contemporaries" (p. i). His thesis, however, exemplifies the old narrowness:

What is clear is that the special socio-economic characteristics of the engineering, building, coal, and cotton industries in the last quarter of the nineteenth century constituted a situation which made possible the resolution of conflict between employers and employed. Formalized bargaining procedures shifted attention away from the contradiction between legal fiction and social reality inherent in the wage contract, and in masking the disparity between the buyers and sellers of labour contributed to the survival of capitalist relations of production in British society (p. xi).

The assumption which makes his discussion seem so familiar is that industrial relations are to be understood only by looking at what happens in the work place. If modern historians have advanced in the study of the relations into which working people enter, it is in the understanding that what happens at work is most fruitfully examined in the context of the wider political and social relations of the class. So narrow is Burgess' focus that he writes at several points about "the improved bargaining strength of labour after 1850" (p. 306), when many historians would argue that

the class was in a very weak position with any chance of mounting a challenge to capitalism as dead as the Chartist movement. The origins of British industrial relations missing from this study extend beyond working- and middle-class politics, consciousness, and social relations to include the process by which laboring people were stripped of their traditional defenses, craft skills, and—in many cases—self-respect. Missing also are such aspects as the use of violence, intimidation, and the language of menace by both employers and workers.

In looking at the details of union formation and collective bargaining settlements, Burgess has very ably chronicled the occasions essential to maintain industrial peace; in describing the ways managers co-opted working-class leaders to keep the employees in line, he has found a necessary condition for that peace in a free society. His focus is too narrow, however, to come to grips with the causes. This is not the new history of the origins of British industrial relations but rather a very good but incomplete narrative history of the origins of some of the institutional forms which governed those relations in the last half of the nineteenth century.

The debate about the standard of living is also narrow in focus, but it is beset by further difficulties: the lack of data to support any firm conclusions at this time; the lack of agreement about which workers, regions, and years are under discussion; and the lack of any meaningful relation to the satisfactions and happiness of British people of the period. The issues are ideological, and the results can be measured quantitatively in dozens of footnotes and thousands of paperbacks sold to students. The seven essays in Arthur J. Taylor's book were published between 1936 and 1966, and all are readily available. The new material includes an extension of Taylor's 1960 article describing the debate, together with contributions from E. J. Hobsbawm and R. M. Hartwell (with S. Engerman). The new material would have fit admirably into an article for the *Economic History Review*, where it belongs in the interests of scholarship and economy. There is no reason for libraries to pay \$18.50 for the hardback version of this book, nor should students be asked to pay \$10 (only £2.90 in the U.K.) for a paperback. Publishers have been given to complaining about the "failure of the market," but they must bear the responsibility for failing those of us who form the market by a spate of unnecessary books at grossly inflated prices.

THOMAS MILTON KEMNITZ  
Barrington, N. H.

ALAN ARMSTRONG. *Stability and Change in an English County Town: A Social Study of York, 1801-51*. New

York: Cambridge University Press. 1974. Pp. xxi, 254. \$15.50.

Specialists in urban history or population studies will welcome this revised version of Alan Armstrong's thesis on York, especially as this famous county town may represent an international type (cf. Viennot on Dijon) and not merely a neglected English one.

Two chapters are devoted to the social and economic background, then intelligently related to four chapters on facets of York demography in the first half of the nineteenth century. Some may find the former thin, but there are discussions of interest, notably the attempt to apply Rowntree's concepts to the 1840s. The second part is, as Armstrong admits, sometimes a matter of "fragile conclusions" resting on "heroic assumptions." In the absence of access to the original civil registration returns, it could hardly be otherwise. The principal sources are the Census Enumerators' Books for 1841 and 1851 supplemented by (above average) ecclesiastical records, poor-law documents, and a contemporary "Sanatory" table, which leads to a charming cross-tabulation of parish mortality with mean altitude. Do ratebooks or electoral registers survive?

Within the limitations of the basic statistics Armstrong demonstrates agility and balance. His more interesting arguments include: York no less than the industrial towns was predominantly composed of immigrants from the surrounding countryside; mortality rates in the 1840s were comparatively high, indicating that urbanization itself brought serious health problems; the higher classes were already successfully limiting their family size; and superficial similarities in household size between pre-Industrial and mid-nineteenth-century communities can conceal structural changes, particularly a rise in the proportion of kin and lodgers at the expense of servants.

Armstrong humbly forestalls some methodological criticism and rightly underlines the need for more advanced statistical manipulation as well as for guidance from the social geographers, but is perhaps less convincing in defending himself against those skeptical of the new social history. Anderson is the surer standard-bearer.

T. J. NOSSITER  
London School of Economics

J. P. D. DUNBABIN. *Rural Discontent in Nineteenth-Century Britain*. New York: Holmes and Meier Publishers. 1975. Pp. 320. \$19.50.

This book serves its theme by assembling seven detailed monographic studies on radicalism in

rural East Anglia, agricultural unions in Oxfordshire, Scottish farm servants, tenant rights, the crofters' "Land War," and the Welsh "Tithe War." The first two are by A. J. Peacock and Pamela Horn, the rest by J. P. D. Dunbabin. Although the whole century is encompassed, and all of Great Britain, there is no pretence of full coverage. The book succeeds because of some authoritative chapters in which Dunbabin stitches the regional studies together and interprets them. It thus manages both to break new ground and to offer insights that can come only from a broad perspective.

One of Dunbabin's findings is that, throughout the century, supply and demand triumphed over protest and organization; in the eastern counties, for example, where laborers' unions became strongest, wages rose least. Although the supply-demand situation for rural labor strongly favored the employer before 1850—farmers often spread work around the parish as best they could—it was mixed thereafter. Rural protests in the earlier period were basically distress signals; they commonly occurred in hard times, were disorganized, pitiful, sometimes menacing and revengeful, and failed either to evoke a sympathetic response from the national government or to bring tangible benefits to the laborers. After 1850 organization was the rule, and successes came when times were favorable or political support could be obtained. Dunbabin insists that in the late nineteenth century national politics were far from irrelevant to the fate of ordinary country folk. Noting this, as well as the social significance of the new county councils, one can easily grasp why Gladstonian Liberalism established its hold on these people. Except in the case of the Welsh "Tithe War," British rural movements had narrow economic aims, though they did not lack ideology. A sense of righteousness gave spirit to these causes, and it derived much more from the Bible than from socialism or anything else.

DANIEL BAUGH  
Cornell University

MARTHA VICINUS. *The Industrial Muse: A Study of Nineteenth-Century British Working-Class Literature*. New York: Barnes and Noble. 1975. Pp. x, 357. \$20.00.

*The Industrial Muse* is a noteworthy addition to the growing literature on working-class culture, but any comparison with the writings of E. P. Thompson, Raymond Williams, and Richard Hoggart must underscore the defects in Vicinus' book. For all the exhaustive research in local history and folklore collections on which this study is based, it

is narrowly conceived and methodologically traditional. Since nineteenth-century British working-class literature offers few developmental signposts, Vicinus has imposed her own structure by focusing on several distinct and generally unrelated areas—broadside and street ballads, political fiction of the Chartist period, dialect literature, self-educated poets, and music hall entertainment, the last of these relating to the working-class in its origins only. Such diversity has fostered an idiosyncratic process of selection: Vicinus scrutinizes closely the propaganda of the coal miners' unions in the 1830s and 1840s, but she ignores improving literature and late-Victorian radical and socialist writings, and she assesses dialect writing with scant reference to Scotland.

Curiosity value aside, many of the works so industriously brought to light are ephemeral and sometimes unreadable, scarcely confirming the author's contention that "good working-class literature was produced which spoke with a clear and confident voice about and for the people" (p. 4). Their interest lies in their relation to the cultural environment in which they were produced, but, despite avowed intentions, it is just this contextual dimension that the book lacks. Works are subjected to conventional stylistic analysis, their imagery and characters delineated, but the laboring world which they reflect remains dimly in the background. One commendable exception to this pattern is the illuminating chapter on street literature, which enlarges Mayhew's picture and affirms the importance of broadsides in promoting literacy, providing accessible entertainment, and integrating newcomers into the urban community. The author is also particularly informative about the economics of publishing and the difficulties that beset working-class writers seeking commercial success.

Vicinus applies ideological as well as literary criteria in attempting to document "the struggle to create and sustain a distinctive literature in the face of bourgeois economic and cultural control" (p. 2). Extolling those who fomented class struggle and incited hatred of the employers, she tends to disparage the writers who offered no challenge to dominant cultural values. Yet it is by no means clear that the literature of social cleavage was any the more authentic an example of worker self-consciousness than that which advocated self-help or expounded domestic pieties. Furthermore, the taste of the audience itself, not merely the dictation of bourgeois publishers, determined what was written, and nineteenth-century working people preferred their reading matter to be escapist rather than exhortative.

F. M. LEVENTHAL  
Boston University

GEORGE DODD. *Days at the Factories; or, the Manufacturing Industry of Great Britain Described, and Illustrated by Numerous Engravings of Machines and Processes*. Reprint. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 408. \$14.50.

HOWARD M. GITELMAN. *Workingmen of Waltham: Mobility in American Urban Industrial Development, 1850-1890*. Baltimore: Johns Hopkins University Press. 1974. Pp. xvi, 192. \$10.00.

Although the locale of their topics is separated by the Atlantic Ocean, these two books deal with important phases of the work scene. George Dodd wrote in the early 1840s and deserves reprinting. To a large degree he deals with process industries such as brewing, sugar refining, distilling, and tanning. In addition he includes stone cutting, bell founding, tower-clock making, and the manufacture of pianos. Nineteen topics, sometimes related, are covered, and ample space is accorded to each out of the over four-hundred-page total. This allows for numerous illustrations and enough detail in the text to make the process intelligible without becoming tedious. The locale is London and its environs. The style of the book is an easy and familiar narrative which manages to convey many details of the busy commercial metropolis. From the historical contrasts presented, one can easily appreciate the advances brought about in such industries by the Industrial Revolution. The greater number of the articles originally appeared in "The Penny Magazine" and in their day were used in schools, including the engineering class of Kings College. This book remains a unique and valuable reference source.

Howard Gitelman, in his *Workingmen of Waltham*, gives deep insight into the manpower behind industry. He treats the period 1850-90 in one of the earliest Massachusetts industrial communities, on the edge of Boston. His prime focus is "Mobility in American Urban Industrial Development." The site chosen is a good one because the two leading industries are widely diverse: a cotton mill and a leading watch factory. Other smaller industries were largely supportive or derived from the leaders. The leading foreign-born labor force, Irish, is described in detail as it gradually achieves more and more acceptance among the old Yankee stock. Moreover, the community remained small enough during the period of study so that the volume of statistics remained manageable. Beyond the main thrust of the book a clear picture is presented of the process by which figures were derived. There is much notice of the various factors which could not be determined, but which modify the sharpness of all figures. Where inferences have been made they are so identified. The various chapters are readably written and offset the stark statistics. The index consists of only two pages and does not do

justice to the wealth of peripheral material in the text. An appendix of over five pages explains the research methods. A second appendix enumerates all of the occupational classifications. These last not only aid the reader but also make the book an effective guide for other researchers engaged in similar projects.

EDWIN BATTESON  
Smithsonian Institution

EDWARD ROYLE. *Victorian Infidels: The Origins of the British Secularist Movement, 1791-1866*. Totowa, N.J.: Rowman and Littlefield. 1974. Pp. viii, 357. \$19.50.

The Victorian infidels of Edward Royle's impressive study were those mid-nineteenth-century secularists who, under the leadership of George Jacob Holyoake, attempted to accommodate English radical unbelief to Gladstonian liberalism after the collapse of Chartism and Owenism in the 1840s. In contrast to the militant deists and atheists who had dominated the freethought movement from the time of Thomas Paine, Holyoake and his followers believed that the political and social realities of the 1850s called not for confrontation but for positive cooperation, even with Christian reformers. As a consequence, the stridently sectarian and millenarian tradition of unbelief in the utopian schemes of Owenite socialists and other early nineteenth-century radicals was consciously abandoned in 1851 for a moderate, progressive, realistic, and respectable future under the aegis of "Secularism." The ideas of the new movement which were largely embodied in John Stuart Mill's essay, *On Liberty*, and promoted by the numerous secular societies and journals that appeared in the 1850s form the core of Royle's book.

Although *Victorian Infidels* concentrates on the middle decades of the century and the role of Holyoake, the author also provides a lucid and perceptive account of the English freethought tradition from the Enlightenment to the thwarted hopes of Chartism and Owenism. He describes the ideological and personal conflicts plaguing the freethought movement in particular and radicalism in general. These conflicts, as Royle so well demonstrates, were if anything exacerbated by the rise of Secularism as Holyoake fought to defend his new policies of cooperation against the increasingly bitter and eventually successful attacks of Charles Bradlaugh and the aggressive atheists who captured the movement and its principal organization, the National Secular Society, after 1861.

Royle's book is not only a thoughtful, well-documented account of a neglected aspect of nineteenth-century religion, it is an important contribution to an understanding of Victorian radical-



ism as well. His analysis of ideas is strengthened by a careful discussion of the geographical distribution of secular organizations and the social composition of their membership. Victorian infidels were for the most part skilled, self-educated workers, like Holyoake himself, who supported a number of radical causes. They were most active in Yorkshire, Lancashire, and the northwest, as well as in London.

The author has made effective use of the papers of Holyoake, Bradlaugh, and Owen, among others, but he has relied primarily upon the publications, mainly weekly journals, of the radical freethought and secular societies. These lend themselves best to a study of the ideas and policies of unbelievers as seen through their own eyes, and that is what Royle has emphasized. Near the end of the book he sometimes treats them in a disjointed and cursory manner rather than integrating them carefully into chapters, but this is a minor defect in a very good piece of scholarship.

RICHARD ALLEN SOLOWAY  
University of North Carolina,  
Chapel Hill

THE MARQUESS OF ANGLESEY F.S.A. *A History of the British Cavalry, 1816-1919. Volume 2, 1851-1871.* Hamden, Conn.: Archon Books. 1975. Pp. 519. \$27.50.

The second volume of the Marquess of Anglesey's *History of the British Cavalry, 1816-1919*, covers the years of the Crimean War and the Indian Mutiny. A good half of the work is devoted to a minute reconstruction of individual cavalry actions in those campaigns and, more briefly, to the cavalry's role in the "Arrow War" of 1860 in China and the Abyssinian campaign of 1867-8. The balance of the book, and much the most successful part, deals with the social history of the cavalry at mid-century.

Academic historians are notorious for their condescension to antiquarians for their mindless pursuit of old material. As often, however, academics are quick to appreciate the genuine services that antiquarians perform in bringing useful evidence to general attention. Anglesey's work in its methods and intentions is antiquarian, with all the virtues and defects that the term implies. His purpose is to raise a literary monument to the courage and achievement of the old horse cavalry by describing its exploits and daily life. He does this in a well-written account, larded with extracts from a wide range of printed and manuscript sources and a generous ration of plummy anecdotes. Using a mass of blue-book material he illustrates nicely the harsh conditions of the troopers' life—poorly paid and fed, housed in crowded, badly ventilated bar-

racks, under appalling sanitary conditions (wooden urinals did double duty as wash basins); their mortality rates from disease were twice those of the foulest civilian slums. The richness of detail about the recruitment, training, education, and diet of "other ranks," and about veterinary services, the purchase of horses, and the market for officers' commissions is equally interesting. This is useful antiquarianism.

The accounts of cavalry action, detached from either a coherent campaign history or a systematic argument about the theory and practice of mounted warfare, are another matter. Only cavalry buffs will be pleased with these disjointed fragments, which are meaningless in the absence of a larger framework of narrative or analysis. A "definitive history" of the Victorian cavalry is not the sum of its battles and cannot be achieved by using Fortescue as a model, ignoring the changing political and technological context of warfare. Let us hope in the remaining volumes of this work Anglesey will not content himself with telling stories, but will pose, and try to answer, some substantial questions. A proper monument is made of durable material.

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Richmond College,  
City University of New York

T. J. NOSSITER. *Influence, Opinion and Political Idioms in Reformed England: Case Studies from the North-east, 1832-74.* New York: Barnes and Noble. 1975. Pp. xii, 255. \$22.50.

Those interested in T. J. Nossiter's two short, suggestive articles on elections and voting behavior in *Political Studies* (1970) or his essay on electoral behavior in E. Allardt and S. Rokkan (eds.), *Mass Politics* (New York, 1970), will be pleased to see this expanded version of his earlier work. As before, he views politics in nineteenth-century England as a matter not only of institutions but of process, paying particular attention to how the system worked and how contemporaries viewed it. As before, he sees changes in the process as essentially linear: from traditional patronage politics in which long established patterns of deference prevailed, through market politics in which the vote was treated as an economic asset, to the politics of individual opinion in which each voter considered political issues alone. And as before, Nossiter emphasizes the importance of retail merchants—the "Shoperacy"—as a major instrument of radical politics because they had the collective ability, motive, and opportunity to press individual opinion in the alien environments of patronage or market politics. He also reiterates his belief that the study of electoral politics should concentrate on

investigations of individual voters to establish the social basis of voting behavior. Finally, he continues to argue that aggregate, statistical analysis can be used to illustrate the progression from predominantly local to regional-centered politics.

These are ambitious themes and important ones in understanding nineteenth-century political life, but they are not presented as part of a larger unity. One of the problems Nossiter was unable to overcome was orchestrating his thematic material to give a central, comprehensive focus to his study. Nor is it clear that Nossiter has added significantly to his earlier conceptual or methodological points. What he does offer to the reader (or rather what his publisher offers for a substantial price) is a more leisured discussion of his arguments and a greater abundance of local detail as evidence to support them.

V. M. BATZEL  
*University of Winnipeg*

BRIAN JENKINS. *Britain & the War for the Union*. Volume 1. Montreal: McGill-Queen's University Press. 1974. Pp. 315. \$12.50.

In this first volume of a projected two-volume study, Brian Jenkins interweaves developments in the Confederacy, the North, Canada, and Great Britain to show how British leaders managed to maintain neutrality during the first year of America's Civil War. By May of 1862, however, they were faced with serious handicaps in the form of Canada's vulnerability, evidence of a breakdown in the effort to maintain a common front with France, unsettled conditions in Europe, and pressure from various segments of the British public.

The author relies heavily on the studies of other scholars, published documents, collections of letters, diaries, and memoirs, and he supplements these with newspapers and manuscripts in public archives and private collections in Canada, Britain, and America.

Most of the themes stressed are familiar: the South's "King Cotton" strategy; British insecurity in regard to Canada; confusion about the purpose of the war; the tension between Britain and the North over the Proclamation of Neutrality and methods of restricting trade; conditions and attitudes among the British working class; reasons for the reluctance of Palmerston and Russell to recognize the Confederacy. The author adds little to the insights and conclusions set forth in older studies by E. D. Adams, Frank L. Owsley, Robin Winks, and others. His work is significant, insofar as he integrates these themes, showing how the events in one center affected developments in the other countries. It is a challenging task and sometimes

the "seams" show; but, on the whole, the result is good.

The most serious shortcoming of the book for the student of this period is the manner of documentation. Endnotes are used whenever the author includes quoted phrases or sentences (often unnecessarily), but there are few citations of sources on which he bases important observations when no quotations are given. When he quotes from letters or other documents cited in secondary sources, he often fails to mention the original depository. Also, it would be helpful to have a listing of the manuscript sources.

The study will be of interest to the general reader because of the way in which it brings together so many of the threads of diplomacy during the first year of the Civil War. If Volume Two has not yet gone to press, one would hope that the strengths of this first book can be maintained while the defects in documentation can be corrected. It would then be of more value to the professional historian.

ALICE O'ROURKE  
*Rosary College*

D. W. FORREST. *Francis Galton: The Life and Work of a Victorian Genius*. New York: Taplinger Publishing Co. 1975. Pp. x, 340. \$14.95.

If the distinction between right- and left-brain dominance has any truth, Francis Galton was surely a left-brain man. Rarely has the urge toward quantification, purportedly controlled by the left brain, been more fully realized. In nearly every situation Galton found something to count: fidgets per minute in a restless audience, geographical distribution of "pretty girls" in the British Isles, brush strokes in a painting. In the words of D. W. Forrest, Galton's new biographer, "Not all of this work was pursued to very definite or useful conclusions, but it is worthy of mention on account of Galton's method, which was to measure whatever and whenever he could" (p. 183). Eventually Galton did settle on a problem worthy of his zeal, that of reducing the laws of heredity to mathematics. As it turned out, his statistical method was less immediately useful for understanding the mechanisms of heredity than the simpler ratios of the Mendelians. Even so, by the time of Galton's death in 1911 his mania for counting had brought statistics into the mainstream of biology and won him the highest honors of the scientific world. The present study adds to Galton's reputation by documenting the sheer volume of his work, and, within certain limits, brings him to life as "not only a genius, [but] also unmistakably a Victorian" (p. 288).



Writing a sympathetic but honest biography of Galton could not have been an easy task, since both his domestic and scientific behavior ranged from the brilliantly idiosyncratic to the suspiciously bizarre. Personally snobbish and overbearing, Galton's daily life was a mirror of Victorian faults; professionally a maverick, he transgressed disciplinary lines with little sensitivity to those he brushed aside. More seriously, Galton's assumptions were racist to a degree seldom matched in the work of other great men of Victorian science. Altogether Galton would be an unattractive subject were it not for his genuine achievements and mad humor.

Forrest excels in describing the essential aspects of Galton's life, private and professional. The summaries of Galton's enormously varied researches (geography, meteorology, biology, anthropology, psychology, statistics, fingerprinting) are accurate and succinct. And despite obvious regard for his subject, Forrest does not flinch from identifying Galton's personal faults. Nonetheless, this valuable work is marred by an overly narrow and chronological approach to biography. While provided with a great deal of information about Galton, the reader can only infer from passing remarks the location of Galton's work with respect to longer-term developments in English science and politics. Galton's place in the history of genetics is not well defined; nor are the political origins and intentions of his eugenical activities taken seriously. These deficiencies would be less striking if other historians had not already developed such material. Unfortunately, Forrest does not refer to their work. Thus, praise of this study must be qualified: within the limits of his approach Forrest succeeds admirably; outside them he provides only clues.

SANDRA HERBERT  
University of Maryland,  
Baltimore County

HUGH MCLEOD. *Class and Religion in the Late Victorian City*. Hamden, Conn.: Archon Books. 1974. Pp. xii, 360. \$16.50.

Here, without preliminary fanfare, is the best single volume on Victorian religion. The particular subject matter of the book is fairly precise. It concentrates on the Christian denominations in London between 1880 and 1914, and re-enforces its general analysis of working-class and suburban religion in the metropolis with particular studies of Bethnal Green and Lewisham. The conceptual frame of the book is equally clear. Hugh McLeod is concerned not with religion and the churches in

themselves, but with the social conditioning of religious behavior. Yet having delimited his subject and defined his concern with it, he brings to bear upon it broad reading in the religious history of different periods and countries, as well as in fiction. He displays a sensitivity to the nuances of behavior within the finely graded hierarchy of the English class structure and a talent for succinct analysis, unhurried as he peels back the layers of custom and conscious intent, bold when he has an argument to advance or a conclusion to draw. He expresses himself with unassuming calmness.

There is much to savor in this book apart from its thesis. The book, in fact, can serve as a superb introduction to its general subject. At the same time, the book presents a sustained and provocative argument: that the seeming strength of mid-Victorian Christianity depended upon the social code of "respectability," a code which possessed coercive force among the middle, especially the upper-middle, classes; that the Victorian working classes, on the contrary, tended to seek consolation for the miseries of their existence "by withdrawing into a more local world, within which their words and actions *were* of some consequence" (p. 282); that religious observance began to give way after 1880 as the code of respectability lost its binding force; and that the appeal of religious associations to ambitious individual spirits among the working class was undermined and eventually all but destroyed by the social code of class solidarity, a weapon almost as useful for socialists as respectability had been for evangelicals.

PETER MARSH  
Syracuse University

WILLARD WOLFE. *From Radicalism to Socialism: Men and Ideas in the Formation of Fabian Socialist Doctrines, 1881-1889*. (Yale Historical Publications, Miscellany, 104). New Haven: Yale University Press. 1975. Pp. 333. \$17.50.

This is a probing and imaginative study of the shaping of socialist ideas among the early Fabians: George Bernard Shaw, Sidney Webb, and to a lesser degree Annie Besant, Sydney Olivier, and others. But Willard Wolfe also makes a general argument about the nature of British socialism. He begins with a discussion of John Stuart Mill and his important influence in shaping a non-doctrinaire socialism, then turns to the revival of socialist thought through the influence of Henry George and the formation of the Social-Democratic Federation. But he devotes the greatest part of the book to comparing the early Fabians, and Wolfe is particularly conscious of the influence of Comte and Positivism upon their ideas. He de-

monstrates that Fabian collectivism grew naturally from British radicalism and that both helped shape the limited socialism of the Labour Party. The argument is well done and supported by an extensive use of manuscript sources. Yet again, one is made vividly aware of both the attractiveness and irritating qualities of the traditions of British moderation.

Because continuity tends to submerge change, the legitimate emphasis on the similarities between radicalism and socialism runs the danger of oversimplifying the comparison. There is also too much emphasis on the quasi-religious nature of British radical and socialist thought. The nineteenth century was an age when people were obsessed with religion, but Wolfe, through the excessive use of such terms as conversion and faith, seems to be arguing that his figures' political views were, in large part, substitutes for religion. Undoubtedly, their political and social ideas filled some emotional needs, and religious terminology, for Fabians as well as more popular leaders, was an obvious way to make concepts immediate. But Fabian socialism was not only, as Wolfe argues, a better faith than radicalism; it appeared to stand a better chance of solving Britain's problems. Wolfe's approach provides many new insights, but perhaps he has not given sufficient attention to the degree to which the Fabians were reacting to the world around them, seeking not a faith but a cure. He is also a little too anxious to point out his own originality and the errors of others. But his book is strong enough not to require such prods to the reader. It is a highly important contribution to the story of the development of left-wing thought in late-nineteenth-century Britain.

PETER STANSKY  
Stanford University

A. J. A. MORRIS, editor. *Edwardian Radicalism, 1900-1914: Some Aspects of British Radicalism*. Boston: Routledge and Kegan Paul. 1974. Pp. x, 277. \$18.00.

That Edwardian subjects remain popular is evidenced by the serialized television dramatization of Galsworthy's *Forsyte Saga* and the interminable *Upstairs, Downstairs*. Perhaps this is as it should be; for a generation as battered and jaded as our own instinctively turns for consolation to a past where problems were simpler, where the lower orders knew their place, and where all was right with the world. Yet nostalgia is, after all, only escape into a mythologized world of wish-it-would-have-been.

The fourteen years separating the "dirty little war" against the Boers from the infinitely dirtier Great War found British society strife-ridden and

beset by controversies ranging over every aspect of domestic and foreign policy. Playing a significant part in these controversies were the radical Liberals, that is, those in the Benthamite tradition of middle-class dissent from an Establishment dominated by the aristocracy and Church of England. Edwardian radicalism is a complex subject that has generated a vast literature. Although the complete story has yet to be told, this book will surely play a part in the telling. The editor has assembled fifteen essays, blending the work of some of the keenest young historians with that of seasoned veterans. Each contributor directs a beam at a facet of the radical phenomenon, thereby helping to illuminate the whole. As regards social reform, G. H. S. Jordan and Roy Douglas deal with old-age pensions and the land question. Stephen Koss, A. J. Lee, A. K. Russell, and A. J. A. Morris contribute essays on political subjects: Non-conformity and the Liberal Party, the radical press, Liberal party organization, and Charles Trevelyan. John Grigg, Catherine Ann Cline, E. C. Moulton, and Howard Weinroth explore foreign affairs, focusing on Lloyd George and the Boer War, the journalist E. D. Morel, radical attitudes toward India, and the issue of nationalism. My own favorites are Dame Margaret Cole's perceptive essay on H. G. Wells' conflict with the Fabian leadership and Marvin Swartz's on the radicals' reaction to the beginning of war in 1914. F. N. Leventhal deals masterfully with H. N. Brailford's search for a new international order—the time has come for a full-scale biography of this influential journalist. Clive Trebilcock's "Radicalism and the Armament Trust" is an eye-opener; apparently armaments manufacturers were not the calculating Molochs of radical propaganda. I for one eagerly await Trebilcock's book on the armaments industry.

These essays are not without their infelicities. Marvin Swartz, for example, asserts that Norman Angell regarded war as "uneconomical." Really! The author of *The Great Illusion* considered war absolutely catastrophic in an interdependent industrialized world. Moreover, a solid essay on the radicals and the suffragettes might have gone a long way toward resolving a particularly vexatious problem. But on the whole, these criticisms are minor; and A. J. A. Morris has rendered students and interested general readers a service.

ALBERT MARRIN  
Yeshiva College

ROSS MCKIBBIN. *The Evolution of the Labour Party, 1910-1924*. (Oxford Historical Monographs.) New

York: Oxford University Press. 1974. Pp. xviii, 261. \$18.50.

This important monograph demonstrates persuasively why the Labour Party replaced the Liberals as the principal party of the left after the First World War. By carefully studying Labour's formative years—the activities of trade councils, labor representation committees, and later constituency parties as well as those of the Head Office—Ross McKibbin shows how surprisingly limited were the changes made by its 1918 constitution. He argues that most historians have been off the mark in seeing World War I as the main determinant of Liberal decline and Labour success. Instead, he makes a convincing case for the enduring antebellum character of the Labour Party, illustrated by its continuity of leadership and personnel, its essential continuity of policy, and especially its continuity of organization. Inextricably linked to the trade-union movement, the Labour Party was national in character and centralized in form. The new constituency parties were successful mainly as they reflected trade-union strengths. Attempts at "federalization" of the party, opposed by the great unions, were likewise rejected by party leaders. Centralization was limited only when it conflicted with the interests of the unions. The war accelerated rather than generated a tendency for the Labour Party to base its strengths not upon broadly articulated principles but upon intense class loyalties and class consciousness. In such a situation, particularly after the further expansion of the franchise in 1918, class self-awareness meant that Labour achieved significant gains at the expense of the Liberals, who could make no particular claims on the loyalties of any class. Within the Labour movement, such loyalties overshadowed socialist doctrine, so that despite postwar rhetoric the movement itself, and service to the movement, became a substitute for any firmly held ideology. In effect, McKibbin concludes, the criticism that the Labour Party has never served the cause of socialism or the socialist vision of the true interests of the working class is wrongheaded. The Party was never designed to do so.

HENRY R. WINKLER  
*Rutgers University*

PARTHA SARATHI GUPTA. *Imperialism and the British Labour Movement, 1914-1964*. New York: Holmes and Meier. 1975. Pp. xviii, 454. \$25.00.

This is an altogether admirable study of British Labour's attitudes toward empire from the end of the First World War to the present. The author sets his problem by quoting Lenin who in 1920

wrote that the English craft unions were "imperialist minded and imperialist corrupted." Gupta rests his argument primarily on unpublished sources in the Public Record Office, the Colonial Office, the Dominions Office, the India Office Library, and the National Archives of India. He has shown in some detail how the anti-imperialist idea became dominant in British Labour circles.

Since its beginning the British Labour Party had both an imperialist and an anti-imperialist wing. Fabians and moderate trade-union leaders belonged to the former, while the Independent Labour Party militants and left-wing elements constituted the latter.

Lenin failed to understand that his model of imperialism, which he conceived of as the last stage of a bankrupt capitalism, would be less relevant if democratic socialist policies prevailed. It was actually not the craft unions that were pro-imperialist. British economic troubles in the twenties were due to an overvalued currency rather than to depressed wages caused by foreign competition. Trade unionists did not look to policies of imperialism in the Kipling mold for an answer. They did pressure Labour governments in the case of Indian cottons and textiles, but the radical anti-imperialist tradition triumphed.

Bevin was the only Labour leader who thought the British craftsman's prosperity depended on empire. In 1965 Labour's acceptance of the idea that military defense demanded empire was really a carry-over from wartime attitudes. Attlee's imperialism seems to have been mostly a matter of prestige. The Fabians were essentially racist, and this caused them to misunderstand the situation in West Africa. "Though the conduct of Bevin and Morrison in these post-war years," Gupta writes, "showed that imperialism could become linked with social democracy and appear to be essential to the latter, it remained true that in the final analysis reformist social democracy neither needed nor could afford an imperial policy."

MARK NAIDIS  
*Los Angeles Valley College*

STEPHEN LYON ENDICOTT. *Diplomacy and Enterprise: British China Policy, 1933-1937*. Vancouver: University of British Columbia Press. 1975. Pp. xv, 209. \$15.00.

This is a brief, yet detailed, examination of the bureaucratic infighting behind British Far Eastern policy. Set against a backdrop of fading hopes that international cooperation, not militarism, would govern world affairs, the narrative contends that the Treasury Ministry's triumph over the Foreign Office was crucial in determining Great Britain's

response to Japanese expansion in China; that is, Britain's shift after 1931 from a policy of placating Japan to one emphasizing financial assistance to China is shown to have been the product of the favor bestowed by the Cabinet on the Treasury's formula. The Foreign Office, worried by adverse turns of events in Germany and concerned lest Japanese ambitions ultimately threaten vital imperial interests in Asia, had steadfastly urged an accommodation with Tokyo. The Treasury's victory, Endicott believes, was the result of the government's susceptibility, in the midst of a great social and economic crisis, to pressures exerted by business firms having a stake in the China market.

The outcome (a policy that wavered uncertainly in the early 1930s) is condemned on all counts: the uncertainties scarcely soothed Far Eastern tensions; London's assistance to the Kuomintang-Nationalist government only served to prop up a cruel militarism, undeserving of support; and Great Britain herself managed to antagonize the Japanese without getting anything in return. The research producing this indictment is developed within the framework of the "revisionism" that has colored in recent years much of the writing on American foreign relations. Endicott is most persuasive when he speaks to the narrower products of his research, namely, his discovery that a bureaucratic rivalry had an impact on policy, and that capitalistic interests were important pressure groups influencing policy. His study is much too limited, however, to sustain some of his broader polemics on the sins of private capitalism.

BURTON F. BEERS  
North Carolina State University

MARY PROUDFOOT. *British Politics and Government, 1951-1970: A Study of an Affluent Society*. London: Faber and Faber; distrib. by Humanities Press. Atlantic Highlands, N.J. 1974. Pp. 240. \$12.50.

The belief that they were finally the masters surged through every Labour breast in 1945. Conservatives cringed at the thought of becoming a permanent minority. But within six years the Labour Party was out, not to return to Downing Street until a narrow win in 1964. A decisive sweep in 1966 was followed by an unexpected defeat four years later. Mary Proudfoot's useful summary of events concludes that by 1970, "There was very little . . . to choose between the two Parties." Perhaps this is one of the reasons for the indecisiveness of the two elections of 1974, the increase in Liberal votes, and the emergence of Welsh and Scottish nationalism.

During the dark days of World War II even *The Times* was prepared to agree that political equality

must not be "nullified by social and economic privilege." Though Proudfoot believes that she is writing about an "affluent society," she neglects the extent to which wealth is concentrated in a small number of hands, while equality, social justice, and educational opportunity are denied to many citizens. The "other" Britain is as evident as the "other" America.

Proudfoot's assessments of prominent political figures are always interesting, but they tend to be rather thin. She excuses all because, "The options open to the British in the fifties were few indeed." There is no sustained analysis of how the British establishment rallied after 1945 to retain its power and wealth and to prevent the move to a more truly democratic society.

When one considers the daring and imagination of Britain's scientists and men of letters in the postwar era, one is appalled at the timidity of British political leaders. As a social democratic alternative to both capitalism and communism, Britain might well have pointed the way to the future, just as she did when she became the workshop of the world and a parliamentary democracy.

THOMAS J. SPINNER, JR.  
University of Vermont

NIGEL NICOLSON. *Alex: The Life of Field Marshal Earl Alexander of Tunis*. New York: Atheneum. 1973. Pp. xiii, 346. \$10.00.

Here, at last, we have a sympathetic account of Field Marshal Earl Alexander written by a former young captain in British army intelligence, later an M.P. under Alexander as defense minister, who saw "Alex" first at the Tunisian capitulation. The sometime Governor-General of Canada was badly served by the John North-edited *Memoirs* (1962) which were properly to be called "Lord Alexander Revisits the Battlefields." General W. G. F. Jackson, a major historian of the Italian campaign, wrote a careful appraisal of a dutiful soldier in *Alexander of Tunis as Military Commander* (1971). In Nicolson's book, well-based on documents, we have an adulatory biography of a proper guardsman who can be seen as a warm person behind his aristocratic British reticence. As his biographer, Nicolson was lucky that Alexander wrote memoirs and preserved letters of his early life and that, when the pressures of World War II came along, he was high enough up the chain of command that the official papers replace the fullness of his former sources. Alexander was present for the retreat to Dunkirk; he pulled the British Army out of Burma; and finally he served in the sticky job of being Montgomery's Commander-in-Chief in the

Western Desert, North African, and Italian campaigns. In the latter he stayed after Montgomery's departure. Although never quite at home as a politician, Alexander went on to be Governor-General of Canada and then Minister of Defense in the 1951 Churchill government.

ROBIN HIGHAM  
Kansas State University

T. M. DEVINE. *The Tobacco Lords: A Study of the Tobacco Merchants of Glasgow and Their Trading Activities c. 1740-90*. Edinburgh: John Donald Publishers, 1975. Pp. xi, 209. £8.00.

Rather than provide a comprehensive and focused survey of Glasgow's tobacco trade between 1740 and 1790, T. M. Devine divides his work into sections which deal with specific questions: who were the tobacco merchants? how many were there? what did they do with their profits? why were they successful? how much did their business suffer because of the American Revolution? The analysis begins with a study of the merchant community. Devine demonstrates that after 1740 most tobacco merchants came from middling, not poor, backgrounds, often from families of other merchants. While new men could enter the trade, the number of tobacco merchants was small, and three or four firms generally controlled half the trade or more. This section of the work, which also includes details on specific firms, is the most significant, but could have been improved greatly by comparisons with other trades, ports, and an earlier time period.

Devine's eclectic study makes three additional contributions. First, he reaches the modest conclusion that investments by tobacco merchants had a limited but significant impact on the growth of particular Scottish industries. Estimates of the total investment in the Scottish economy and the growth of that economy would have been helpful, but Devine's painstaking investigation of investment patterns of particular firms and in particular industries is commendable and takes one as far as this methodology can. Second, Devine demonstrates that most financing of the Scottish tobacco trade was done by private individuals. He contradicts the view that banks played the major role. Third, the author shows that Glasgow's trade with Virginia did not collapse because of the American Revolution. After 1783, stores were re-established in the Chesapeake, and Scottish merchants shifted from the re-export to the direct-carrying trade.

PAUL G. E. CLEMENS  
Rutgers University,  
New Brunswick

PETER BERRESFORD ELLIS. *Hell or Connaught! The Cromwellian Colonisation of Ireland, 1652-1660*. New York: St. Martin's Press, 1975. Pp. 268.

T. C. BARNARD. *Cromwellian Ireland: English Government and Reform in Ireland, 1649-1660*. (Oxford Historical Monographs.) New York: Oxford University Press, 1975. Pp. ix, 349. \$29.00.

The two works under review deal with approximately the same decade in Irish history (a period from which Irish historians seem instinctively to recoil), but otherwise they are as different as chalk is from cheese. The main themes of Peter Berresford Ellis' book are concerned with the central topics of the 1650s: with the vast agrarian upheaval caused by land confiscation, plantation, and transplantation; with religious persecution; and to a lesser extent, with the impact on the Irish administration of political changes in England. This is narrative history written in great detail but anecdotal and wayward in its course. Though he modestly disclaims an "academic status" for the work and eschews footnotes, it is evident that he has read widely and, through some acquaintance with contemporary Irish literature, provides a welcome if somewhat opaque window into the Gaelic world. Unfortunately he makes no attempt to evaluate his sources and curiously does not refer to R. C. Simington's great sets of source material concerning the Catholic Irish landowners and their Cromwellian supplanters.

T. C. Barnard's book, in contrast, has little to do with "Hell or Connaught" and therefore might be considered peripheral to the central issue; but it is a most original contribution to the history of the 1650s. In spite of the destruction of most Irish Commonwealth records he has tapped a vast amount of source material. Indeed his industry has been truly prodigious.

After the Cromwellian conquest Ireland was virtually a *tabula rasa* on which the English parliament sought to imprint its own ideas on government, central and local; on finance and trade; and on religious, educational, and legal reform. When Henry Cromwell took over government from the military commanders in 1655 the harsher measures of the military administration were ameliorated, but the new policies of the civil administration equally failed in their main purpose to "anglicize" the native Irish population and to make them Protestants. Nor could they impose uniformity among Protestants themselves. Barnard indicates the reasons for failure—notably that, since the Irish parliament had been abolished, legislation for Ireland had to be passed through the English parliament. Inevitably its interest in Ireland was intermittent, lacked continuity, and was subject to shifts of power in London.

Before the rising of 1641, the Protestant element,



mainly administrators who had come over from England in the late Tudor and early Stuart periods, though not the major landed interest, had a near monopoly of political power. Barnard shows that this element (the "old Protestants") came back into favor under Henry Cromwell at the expense of the Cromwellian planters (the "new Protestants"). They facilitated the restoration of Charles II and thus perpetuated their own survival as the "Protestant Ascendancy." In these transactions a key figure was Roger Boyle, Lord Broghill. One of the novel features of Barnard's book is the space devoted to the present fashionable history of ideas on education, on "the advancement of learning," and on the rise of science in the early seventeenth century. Here too Broghill was a key figure. Making use of the recently available Hartlib papers, Barnard demonstrates substantial Anglo-Irish support for these ideas. This support lasted into the Restoration period and led to the establishment of the Dublin Philosophical Society, which was the forerunner of the present Royal Dublin Society. Thus, in spite of the cataclysmic nature of the Cromwellian catastrophe, one is reminded of the continuities in Irish history. This is a most stimulating book.

DONAL F. GREGAN  
St. Patrick's College,  
Dublin.

MAURICE R. O'CONNELL, editor. *The Correspondence of Daniel O'Connell*. Volume 3, 1824-1828. New York: Barnes and Noble. 1974. Pp. vi, 441. \$30.00.

There is little to say about this volume in Maurice O'Connell's edition of Daniel O'Connell's letters, not because of any flaws, but because of its excellence. As nearly every reviewer of previous volumes in this series has noted, O'Connell is a skilled editor, economical in his interpretative notes, yet fully adequate in his explanations. The production of this volume by the recently revived Irish University Press is excellent.

The years covered here, 1824-28, are of great importance both to Daniel O'Connell and to Ireland, for they were the time of the establishment of the Catholic Association and O'Connell's almost singlehanded creation of the modern Irish nation. O'Connell's letters to and from bishops and leading politicians are fascinating, but even more interesting is his domestic correspondence. In his letters to his wife one finds delightful evidence of a fact too easily ignored: O'Connell, the hard-headed political strategist, was when necessary an immensely charming man—gentle, courtly, witty, and solicitous. Undeniably, his control of the masses led directly to his political ascendancy, but his charm and unction in the drawing rooms of the

Catholic gentry and merchants were important to the financing and functioning of his movement.

The only problem this fine volume raises is the fear that before the entire series is completed the economics of present-day scholarly publishing may force its discontinuance or a deleterious alteration in its format.

DONALD HARMAN AKENSON  
Queen's University  
Kingston, Ontario

W. S. SCOTT. *Jeanne d'Arc*. New York: Barnes and Noble. 1974. Pp. 239. \$13.75.

BLAISE DE MONLUC. *The Habsburg-Valois Wars and the French Wars of Religion*. Edited by IAN ROY. (Military Memoirs.) Hamden, Conn.: Archon Books. 1972. Pp. x, 253. \$9.00.

Though they deal with very different personalities, these two books are similar in some respects. One is the autobiography of a military hero; the other, a biography of one of the few military heroines in history. Both characters are slightly overblown by their respective authors.

Joan of Arc has captivated the imaginations and the pens of numerous historians and biographers, though her career spanned only two and a half years, and her actual accomplishments are questionable. In 1973 and 1974, three biographies of the Maid appeared in English, including Scott's, which is one of the most puzzling. Scott, a retired English priest, appears to have devoted his entire life to a study of Joan, a study which has not proved particularly productive for historians. The book is charming in its way, filled with antiquarian facts, scholarly arguments about the routes Joan took, and local legends (the words "there is a strong local tradition that . . ." appear frequently in the text). What is missing is a meaningful appraisal of Joan that fits her squarely into the era when she lived. In his generally admirable annotated bibliography, Scott lists only one book dealing with the general history of France during the Hundred Years' War, and it is almost a century old. Works such as those by E. Perroy are notably absent, perhaps because Perroy believes that Joan was a far less significant historical figure than does Scott.

Scott is guilty of perpetuating all the clichés about Joan. He views her as a patriot fighting to save the French nation as well as Charles VII; he regards her as a pure, almost angelic girl; and he believes that the outstanding feature of Joan's character was that she was "an individualist."

Scott's lack of broad vision, and his over-concentration with the minutiae of the Maid's life, have created a one-sided, over-glamorized por-

trait. Had he stopped to consider what Joan was *really* able to accomplish during her brief career, he might have written a more balanced account.

Blaise de Monluc puffs himself out of proportion because his *Commentaries*, written in 1570–71, were designed partly as an attempt to regain royal favor. Thus Monluc emerges from his own pages as a valiant, swaggering, lovable Gascon who is also courteous, kind, resourceful, and obedient to those in command. In glorifying his exploits, Monluc was also writing a book to instruct young commanders in the tactics and even civilian machinations of the military life: how to behave at court, how to deal with women (it's best to avoid them), how to conduct a siege or lead a successful retreat. Monluc's autobiography is fascinating, and it is a pleasure to read selections from it in English. Ideally, a new translation could have been provided; but the one presented here, first written by Charles Cotton in 1674, has been modernized and is eminently readable.

The selection of materials presented in this volume is entirely adequate. Approximately one-fourth of the original *Commentaries* has been included, and while one could wish that more of Monluc's descriptions of court life and of the Wars of Religion had been inserted, the reader can find here the general's dramatic account of his defense of Siena in 1554.

The most interesting aspect of this military memoir is the ambivalence Monluc feels for the court. Though he despises courtiers, he is also completely captivated by the prevailing notions of honor and "good repute." Noble deeds on the battlefield are not enough. The way to succeed in the sixteenth-century French army is to ensure that one's exploits are suitably reported to the king or queen.

Ian Roy's introduction is highly informative, his footnotes are succinct and helpful, and his brief biographies and glossary of military terms are extremely useful. This is definitely a book which both military historians and historians of France generally will find valuable and instructive.

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NATALIE ZEMON DAVIS. *Society and Culture in Early Modern France: Eight Essays* by Natalie Zemon Davis. Stanford: Stanford University Press, 1975. Pp. xviii, 362. \$15.00.

As a collection of essays on peasants, artisans, and the illiterate populace of the cities of early modern France, this book should interest historians who are not directly concerned with the period or with popular history. Through what she calls "case studies" of the *menu peuple*, Natalie Davis demon-

strates her impressive and wide-ranging research not only into little-known or used documents of her period but also into the scholarship of the social sciences in general. Though five of these essays have appeared in periodicals, their presence in this volume allows readers to appreciate the coherence of Davis' research and methodology and to acknowledge the significant contribution of her work to opening new vistas on the society of early modern Europe.

Davis clearly demonstrates the importance of collective behavior, playlets, pamphlets, welfare rolls, village festivals, political tracts, and sermons to understanding relationships among people and grasping the cultural traditions and symbols of a period. But this collection is scarcely a mere pot-pourri of popular sources relating to the *menu peuple* and the culture of early modern Europe. As the author points out, she has presented her material according to certain views of social structure and process, which have emerged out of her years of work with the documents. Using the studies of sociologists, anthropologists, and psychologists to shed light on the society and people she studies, Davis has demonstrated the value of the cross-disciplinary approach and has provided scholars with a multidimensional and dynamic scheme of early modern society that acknowledges many more variables in the social order and more complex causes and results of social change than property, power, sex, age, and religion alone or together can encompass.

In these essays, as in all her work, Davis raises questions and poses answers that are crucial to an understanding of this society and point the way for further investigation. She asks what kinds of experience might contribute to the formation of social consciousness among male artisans. What prompted Protestant allegiance among groups, in particular urban women. What political and social use, if any, did public festivals serve. Further she explores patterns of sexual inversion in literature and popular festivals, the shape and structure of popular religious violence, and the goals and actions of the participants in religious riots in sixteenth-century France. She looks for the relationship between written and oral material, which leads her to ask what impact did the establishment of new communication networks have when printing touched popular life in the sixteenth century. To further enrich the readers' understanding of this society and culture, the book is illustrated with eight pages of woodcuts, paintings, and engravings chosen with a combination of Davis' precise scholarship and delightful humor.

Because Davis is interested in social structure, the context of social change, and the relationship between the religious and secular as well as be-

tween male and female, her work will inevitably raise questions for historians of other periods and places. Students of the sixteenth-century Reformation will be interested especially in these glimpses into the lives and values of the men and women—artisans, tradesmen, and craftsmen—who experienced the impact of the changes taking place around them but who have been largely ignored because they left practically nothing in writing.

This work offers scholars two rewards. First, Davis' important contributions to early modern European studies are now collected and edited (in many cases with new footnotes and insights), making them easily accessible. Second, having sampled this *apéritif*, the historian will eagerly await the *pièce de résistance*: Davis' anticipated study of the printers in sixteenth-century Lyon.

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GEORGES FRÊCHE. *Toulouse et la région Midi-Pyrénées au siècle des lumières (vers 1670-1789)*. Paris: Éditions Cujas. 1975. Pp. xviii, 982.

With the publication of this massive study of upper Languedoc by Georges Frêche, students of Old Regime France acquire a wealth of new information on social and economic conditions and their regional diversities. The broad outline of Frêche's findings fit the now-familiar pattern of population growth, price increase, and economic expansion. The particular contribution of this work is to illustrate the details of economic change with, possibly, greater precision than ever before. Though not so profoundly innovative as Goubert on Beauvais or Le Roy Ladurie on lower Languedoc, Frêche has written a model regional study, in which the pursuit of methodological rigor is evident on every page.

Frêche covers this extensive portion of southern France, inhabited by over two million people, by focusing on the material framework in which they lived: the distribution of land, agricultural production, prices, population, taxes, and markets. His approach, in nearly all instances, is to seek the quantifiable aspect of his subject. It should be noted at once that Frêche frequently achieves the highest goal of quantitative history: to pose questions with a meaningful human dimension and answer them as rigorously as possible. The changing professional composition of villages and towns; the number of rural schools, charity boards, and medical personnel; the price of popularly-consumed grains—these few examples may serve to suggest the wide range of subjects Frêche considers. Surely, the author's success in telling us about *people* by counting and calculating derives from his

absolute mastery over an immense variety of documents and research methods. The technical demands of this study were clearly awesome.

The key socioeconomic development in upper Languedoc was its evolution into a center of cereal monoculture for export toward the Mediterranean. "The production of wheat commands all," says Frêche, and he traces the consequences of this to the concentration of land in the hands of the urban rich and the increasing pauperization of the rural population, which was growing even faster than the national average. The author locates the source of economic dynamism in improved market conditions, produced by free-trade policies, by better roads, and, above all, by the construction of the Canal des Deux Mers. Landlords benefitted from high prices, lower labor costs, and a five-fold increase in cereal exports during the eighteenth century.

Frêche has not lost sight of national trends in the richness of his regional data. Readers should find his comparative treatment of prices and subsistence crises illuminating. The extensive methodological discussions will be immensely useful to local specialists.

So thoroughly has Professor Frêche described material conditions in the Midi-Pyrénées that one wants to know more about how people thought and acted. The texture of urban life, the family, and protest are not major concerns here. But our appetite for still more information—coming after 800 pages of dense text—can only be a tribute to the energy, ingenuity, and expertise that produced this work.

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OLWEN H. HUFTON. *The Poor of Eighteenth-Century France, 1750-1789*. New York: Oxford University Press. 1974. Pp. x, 414. \$29.00.

JEAN-PIERRE GUTTON. *La société et les pauvres: L'exemple de la généralité de Lyon, 1534-1789*. (Faculté des Lettres et Sciences Humaines de Lyon, 26.) Paris: Société d'édition "Les Belles Lettres." 1971. Pp. lv, 495.

JEAN-PIERRE GUTTON. *L'État et la mendicité dans la première moitié du XVIII<sup>e</sup> siècle: Auvergne, Beaujolais, Forez, Lyonnais*. Lyon: Centre d'études Foréziennes. 1973. Pp. 248.

JEAN-PIERRE GUTTON. *La société et les pauvres en Europe (XVI<sup>e</sup>-XVIII<sup>e</sup> siècles)*. (L'Historien, 18.) Paris: Presses Universitaires de France. 1974. Pp. 207.

Olwen H. Hufton's study of the poor examines the last four decades of the Old Regime in France, while Jean-Pierre Gutton, in his *thèse de doctorat*, concentrates primarily upon Lyon and its

surrounding region in order to range over two and a half centuries of change in attitudes and institutional responses toward poverty. Both explore the common features of poverty during the Old Regime, seeking to define the threshold between simple poverty and destitution in terms of occupation, diet, health, housing, and family.

Gutton and Hufton find most of the king's subjects dwelling insecurely on the threshold, "*pauvèrisable*." Hufton cites St. François de Sales' telling image of charity as a kind of rich embroidery on the plain fabric of poverty; scholars must look to about 1850, Gutton tells us, for the tide of endemic mendicity to recede. Such conditions lead to "an economy of makeshifts," amplified by Hufton in chapters on relief, begging, theft, smuggling, prostitution, and the family. Gutton shows—"aux origines de l'errance"—how many trifling occupations border on vagrancy, how many *petits métiers* were casual and interchangeable expedients.

Hufton's work will attract the general reader as well as the specialist. Her description of seasonal migration conveys vividly the fatigue and dust of the journey. The reader can imagine the feelings of the poor toward the pious ladies who made them sell their last decent utensil before they might receive domestic charity. The mechanisms of public relief disbursed by intendants in bad years are seen as a contest between local authorities over scraps cast abroad in order to keep up hope of "something coming." Women's roles emerge clearly: the exploited lace maker struggling to amass a dowry; the mother spending her entire wage on child care, directing the ruses of a beggar brood, or obscenely taunting the reluctant almsgiver; the linen thief, the bawd, the decoy for a band of thieves. Hufton concludes the life cycle with the birth of an unwanted child: what must a mother have thought or felt who left her child in a drain, rather than keep it or entrust it to charity?

Gutton's focus on the Lyonnais—rich in its diversity of social milieux, institutions, and local records—allows him to weave large patterns, if on a smaller frame than Hufton. He can, for example, trace various paths of downward mobility in legacies to hospitals and in other fiscal records. He frequently looks beyond Lyon, however, in order to interpret the repeated efforts made in France and in other parts of Europe to achieve a total regulation of the idle, desperate poor, especially by means of institutional confinement.

If indeed the founding of the Aumône-Générale at Lyon in 1534 set a precedent for later moves to confine the poor, Gutton's special contribution is to have traced the undertow against this tide, citing Pascal's desire to die with the *incurables* and Massillon's apostrophe, "Is it not wretched

enough to be reduced to feigning wretchedness?" Royal declarations portray the *hôpitaux-généraux* as places of confinement; their administrators had other purposes, still evincing a refractory spirit when called upon to execute the Declaration of 1724 against vagabonds—hospital registers at St. Etienne reveal connivance with inmates in eluding harsh penalties for recidivism.

Both works have minor shortcomings. Hufton's avowedly qualitative work abounds in estimates and generalizations—deftly drawn contours around a profusion of detail. These invite refinement or revision. In some cases the firmness of line is deceptive. Hufton follows Christian Paultre's gross misreading of the monthly ration accounts of the *dépôt de mendicité* at Bourges, for example, to conclude that there were 3,388 inmates rotting in idleness there, rather than some 115.

Given his major theme, it may appear that Gutton has skimmed important aspects of policy and ideology. The reviewer hoped for a fuller re-examination of the conflict between royal and municipal authorities, which came to a head in the missions of the inspector Jean Colombier in the 1780s. More than a footnote might have been devoted to the efforts of the administrators of *La Charité* to elicit public discussion, via the *Affiches de Lyon*, of such matters as the means for employing the poor confined in hospitals (1769).

Gutton's *État et la mendicité* is based on his *thèse de troisième cycle* of 1967 entitled, "L'exécution de la déclaration royale du 18 juillet 1724 concernant la mendicité: généralités de Lyonnais et d'Auvergne." A model study of policy, administration, and society, it fills an essential gap and includes an account of Trudaine's efforts, while intendant of the Auvergne, to establish workshops to employ the beggars and vagabonds arrested and confined in hospitals under the terms of the Declaration.

Gutton elaborates earlier themes on a European scale in *Société et les pauvres en Europe*. He underlines the socially disruptive effects of poverty and the mentality of social fear. His work and Hufton's highlight the vulnerability of Old Regime society to the depredations of the unruly poor.

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WARREN ROBERTS. *Morality and Social Class in Eighteenth-Century French Literature and Painting*. Buffalo: University of Toronto Press. 1974. Pp. xvi, 188. \$15.00.

Warren Roberts develops the point that the literature and painting of eighteenth-century France reflected developments in French political and so-

cial life, especially developments in the history of the French monarchy and aristocracy. Accepting the now familiar view that aristocracy did not decline but, in fact, revived in the eighteenth century, the author explains the manners and morality pictured in literature and painting as being as much the product of this resurgent, but contradictory, aristocratic society as it was the result of the evolution of artistic forms.

Eighteenth-century French painting and literature reveal three major tendencies: one sentimental and romantic which stressed the Christian view of love, marriage, and the family; another openly erotic, sensual and un-Christian; and a third combining wit, sex, cruelty, and destructiveness with sharp social criticism. Seventeenth- and eighteenth-century aristocratic life provided the models and values for the various types found in this literature and painting.

The author also argues that to the extent that literature and painting revealed bourgeois values, these values and the art forms in which they were expressed grew from earlier aristocratic roots. He finds this especially true of the sentimental-romantic literature and didactic painting which preached the virtues of old-fashioned Christian love and marriage as "bourgeois" virtues.

Roberts' thesis is well argued. Aristocratic art, manners, and morality imposed themselves on bourgeois society. The bourgeoisie criticized the aristocrats, but they also copied them and longed to join them. It would be a mistake to conclude from Roberts' discussion, however, that the bourgeois spirit was *only* a borrowing from the aristocratic spirit. For the Christian morality of love, marriage, and the family, for example, was not the monopoly of the aristocracy (or of any class), and its social expression was not confined to the arts. The Parisian poor also believed, and they were not consumers of art. All classes simply shared a common heritage.

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Proust, Kalker, and Venturi, Lough raises many of the fundamental problems posed by the complexities of attributing authorship to the 79,000 odd entries in the *Encyclopédie* and the *Supplément*. The author surveys briefly the contributions made throughout the span of the vast publishing enterprise by the original "*société de gens de lettres*," delineating these initial contributors from the many other specialists who were recruited by them. Lough is concerned, too, with the sociological origins of the contributors, concluding that it is difficult to go beyond "the obvious fact that the contributors represent a cross section of the upper and middle classes of French society in the period 1745-1765" (p. 51). He rejects as insignificant the findings of Proust and Kalker who analyzed the writings of the surviving contributors during the Revolution, and especially the Terror, and who concluded that these men were "moderate reformers," not radicals. Quite correctly Lough suggests that the intellectual significance of the contributors is to be sought in the context of the first two decades of the reign of Louis XV, when this generation flowered, and not in that of the Revolution in which longevity became the most distinguishing quality of this talented group.

It is in his topical treatment of the authorship of the articles that Lough demonstrates most clearly his mastery of the complex materials. Here some readers may be surprised, for instance, to find that Holbach's main contribution lay in the realms of mineralogy and metallurgy. Yet, Diderot's own preoccupation with science and technology and his concern to propagate the scientific spirit and the Baconian method are, of course, recognized.

If the progress of knowledge today has almost nullified the value of the *Encyclopédie* articles, the writers' attempt to give a rational explanation of the universe remains as impressive a collective effort as ever; and Lough's volume is indispensable for the specialist in assessing their undertaking.

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JOHN LOUGH. *The Contributors to the Encyclopédie*. London: Grand and Cutler. 1973. Pp. 120. £1.90.

In some respects this valuable reference work stands in the same relationship to the monumental *Inventory of Diderot's 'Encyclopédie'* of Richard Schwab as the four-volume text of the *Supplément* to the *Encyclopédie* did to the seventeen volumes of the original work itself. In particular, John Lough's book complements and extends the author index of the known contributors provided by Schwab.

Drawing upon the findings of scholars such as

KEITH MICHAEL BAKER. *Condorcet: From Natural Philosophy to Social Mathematics*. Chicago: University of Chicago Press. 1975. Pp. xiv, 538. \$22.00.

Resisting the temptation to portray Condorcet as the sanguine martyr to Enlightenment ideals, Keith Baker has composed an intellectual biography around a single abstract theme: the philosophe's evolving conception of social science. Much more than the history of a disembodied idea, however, this significant book subtly interweaves Condorcet's published and unpublished thought with



the key episodes of his experience—the early years in Julie de Lespinasse's salon, rise to prominence in the Paris Academy of Science, admission to Turgot's short-lived administration, emergence as permanent secretary of the Academy, and, finally, tragic return to political life during the Revolution. Condorcet's social science depended upon participation of the intellectual in public affairs. Therefore, Baker also expends considerable effort linking Condorcet's thought to the great political and social questions of late eighteenth-century France.

The vicissitudes of Condorcet's mature career convinced him that physical science offered social science the model for creating an enlightened political and social order. The conviction derived from skepticism concerning the certainty of scientific truth. Physical science coaxed Nature into revealing not her hidden essence, but rather her probable workings relative to human observation and analysis. Therefore, could not similar observation and analysis be applied to the conduct of human affairs? Armed with the calculus of probabilities as his appropriate measuring device for evaluating human experience and conduct, Condorcet harbored no sanguine illusions about a scientific elite imposing moral perfectibility upon an untutored world. Physical properties submit far more easily to observation and ordering than do human affairs. Nevertheless, Condorcet believed that in the calculus of probabilities he had found an essential epistemological link between the moral and physical sciences. Thus, for Baker, the climax of Condorcet's work was not necessarily his paean to the idea of progress, the *Esquisse d'un tableau historique des progrès de l'esprit humain* (1793), but rather the *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix* (1785). The *Essai* represented Condorcet's major attempt at reconciling the responsible decision-making of an enlightened elite with the principle of voter consent, the problem which, with later emendations, became the philosophe's enduring legacy regarding political and social thought.

No summary review can do justice to this immensely rich, at times overwhelming, book. It is occasionally repetitious. A few lengthy segments, such as the elaboration of Condorcet's calculus of consent, discussion of citizenship education under the Old Regime, or analysis of the historical evolution of probable belief, might have been summarized more concisely. Beside the virtues of *Condorcet*, however, such reservations degenerate into quibbles. Baker's challenge to Koyré's interpretation of the significance of the Newtonian synthesis for the science of man, the author's analysis of the role of the scientist as social authority, his discussion of the reasons for Condorcet's political failure, and his important statements on the dis-

tinctions between Condorcet and Comte, are extremely suggestive. Baker's *Condorcet* is a superb account of a key Enlightenment figure and his France. It complements Cahen and is a worthy companion to Shackleton's *Montesquieu*, Wade's *Voltaire*, and Wilson's *Diderot*.

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HENRI GRANGE. *Les Idées de Necker*. Paris: Librairie C. Klincksieck. 1974. Pp. 669.

This massive, enthusiastically partisan study of Jacques Necker's ideas has the indisputable merit of emphasizing the solid intellectual substance behind his career. Like his ministries under the Old Regime and the early Revolution, Necker's thought can be damned for a too-careful balance between more exciting alternatives, but we need to be reminded that he was no mere cautious opportunist, but a thinker whose ideas were positive, wide-ranging, and remarkably consistent. The main concern of Grange's volume is an analysis of Necker's intellectual system as revealed in his many books; there is also some attention to the public reception that these books received, and their subsequent influence and modern relevance.

Grange divides Necker's system into economic and social ideas, political theories, and the defense of religion, and demonstrates convincingly the interrelationship of all three, as stemming from the deplorable but inevitable institution of property and the subsequent authoritarian sanctification of exploitation of the poor by the rich. (Here Grange notes Necker's indebtedness to the bitter social theory of Linguet, who deserves more than five and one-half pages of commentary.) The most crucial portion of Grange's analysis is thus his long section on Necker's economic and social theory, with emphasis upon such themes as the appropriation by employers of surplus value, the indispensability of luxury and of a privileged class as a barrier against centralized despotism, the absolute right of the poor to survival and subsistence, and the obligations of an "interventionist" government to assure such subsistence and the common good. Here Grange makes a case for Necker's originality in steering a positive path between mercantilism, aimed at state grandeur and power, and the economic liberalism of late eighteenth-century theorists. Grange then reviews exhaustively Necker's political theories (largely pragmatic and basically conservative) and religious ideas (as a defense of the status quo, and as a basic theological unity); in the latter field the author insists upon Necker's honesty in seeing the Catholic faith as essential to an irrational, mystery-loving French people. In all

of this, Grange demonstrates an admirable range of intellectual and historical grasp, with only rare lapses. For example, though he is right to stress Necker's basic cultural pessimism, recent research demands more subtlety than Grange's reference to the "unshakable optimism" of Necker's contemporaries. Nevertheless, the work is a significant reevaluation of an important historical figure, and a genuine contribution to intellectual history.

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ROLAND MARX. *La Révolution et les classes sociales en Basse-Alsace: Structures agraires et vente des biens nationaux.* (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, 29.) Paris: Bibliothèque Nationale. 1974. Pp. 572.

From 1791 to 1805, the French government sold about one-fifth of the cultivable land in northern Alsace. Of the area sold, 82 percent had belonged to the Catholic clergy, the rest to émigrés; 56 percent seems to have gone ultimately to villagers, many of whom were cultivators, and 44 percent to town dwellers. The business involved 35,000 original transactions and thousands of resales between private persons. The massive documentation in which these facts are recorded, and some questions they suggest, form the core of this book, originally a thesis submitted at Strasbourg in 1969.

Alsace is unique in more ways than most provinces. This book fits usefully into the body of historical writings about the province, but somewhat awkwardly into any larger geographic frame. Marx's interpretation concentrates on totals, averages, and trends for the whole Bas-Rhin, an emphasis which produces a weakness and a strength. The weakness appears in the analysis of local differences, which is sketchy. It could scarcely be thorough and methodical when Marx employs one sample of 139 communes to study local percentages of Catholic church property, another sample of 28 communes to study the social distribution of laymen's property before 1791, another sample of 78 communes to study population increase from 1723 to 1791, and yet another sample of 60 communes to study the social identity of original and ultimate purchasers. These samples range from about 5 to about 25 percent, and overlap in part.

The strength of Marx's interpretation appears in the subtle and instructive discussion of fluctuations in land sales, trends in the property distribution, and the relationships between these and political and religious options. Perhaps most valuable is the large amount of trustworthy quantitative information, which places the Bas-Rhin

with the Nord and the Sarthe among the departments where the revolutionary land sales have been best studied.

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R. R. PALMER, editor and translator. *The School of the French Revolution: A Documentary History of the College of Louis-le-Grand and Its Director, Jean-François Champagne, 1762-1814.* Princeton: Princeton University Press. 1975. Pp. ix, 300. \$15.00.

R. R. Palmer, builder of monumental sculptures, also works in burnished miniatures. His edition of documents concerning the Parisian college of Louis-le-Grand aims his own fifty-year accumulation of historical skill and wisdom at a single institution's half-century encounter with a revolutionary era. The documents, with extensive comments, trace an elite public boy's school from the expulsion of its Jesuit masters and consequent reorganization in 1762-64 through its enthusiastic response to the Revolution—it spent eight years as Equality College and was the only school to remain open continuously through the Revolution—to an inspection tour by First Consul Napoleon Bonaparte in the Year IX; Palmer takes the story to the empire's end via his own summaries and documents concerning the reorganization of educational institutions in general. Along the way we encounter such telling details as the award of a scholar's prize to Maximilien Robespierre in 1781 and the principal's account of a student "insurrection" in 1792. The documents and commentary emphasize the administration and educational thought of Champagne, a Louis-le-Grand graduate and former subdeacon, who directed the school through most of the Revolution and empire. The book closes with Champagne's "Ideas on Public Education" (1790) and a large segment of his "Views on the Organization of Public Instruction . . ." (1800); the two essays provide clear statements—the first more sweeping than the second—of a conception of education as public, egalitarian, and intensely rational. Champagne's proposals of 1800 anticipated the Napoleonic educational reforms, whose effects are still evident today, in several important regards. In Palmer's presentation of the educational contribution of J.-F. Champagne, we witness a marvelous coalescence of author, subject matter, and editor.

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WILLEM FRIJHOFF and DOMINIQUE JULIA. *École et société dans la France d'Ancien Régime: Quatre exemples Auch, Avallon, Condom et Gisors.* (Cahiers des An-

nales, 35.) Paris: Librairie Armand Colin, 1975. Pp. 118. 32 fr.

R. D. ANDERSON. *Education in France, 1848-1870*. New York: Oxford University Press for Clarendon Press, 1975. Pp. 289. \$25.75.

PAUL RAPHAEL and MAURICE GONTARD. *Hippolyte Fortoul, 1851-1856: Un ministre de l'instruction publique sous l'Empire autoritaire*. Paris: Presses Universitaires de France, 1975. Pp. 344.

As educational systems and practices have been called upon to fulfill a wider range of functions in the modern world, historians have given more attention to the history of education. Enough searching and thorough work has appeared in the past quarter-century to make it unnecessary for the historian in other fields to rely upon myth or cliché when discussing the place of formal education in past eras. The works under review are solid examples of this increasingly significant genre.

*Ecole et société dans la France d'Ancien Régime* is an essay in historical sociology which examines the data relating to students who enrolled in the secondary schools of four small French cities during the eighteenth century. The authors are concerned to show the students' social origins, the retention and completion rates, the percentage of those who continued their education beyond secondary school, the social mobility of those who attended, and other socioeconomic indicators such as the relationship of enrollment to wheat prices. The brief text is supported by fifty-two graphs, tables, and maps based upon patient and imaginative uses of baptismal, marriage, school, and tax records. The authors are not given to global statements. They support Dainville's findings that there were more lower-class students in these schools than one might have expected. But they add that the opportunities for such schooling for the sons of artisans and peasants seemed to be closing in the years immediately before the Revolution. They find no significant social mobility related to such schooling: the lower-class sons who completed the schools ended in the priesthood, and their mobility had no demographic effect. The authors conclude that the relationship of secondary schooling to social mobility is one which simply assures the social stability of those who take advantage of it, and the pattern persisted through the nineteenth century.

*Education in France, 1848-1870* covers every level of formal education and its practical and theoretical relationship to the state and to society. The Ministry of Education during the Second Republic hoped to achieve the often aborted goals of equality and fraternity remembered from the Revolution of 1789. Under Napoleon III it was the fear of those goals which occasionally united liberal bourgeoisie and Catholic conservatives in their ef-

forts to retain an educational system not much different in intent and practice from that of the era of Napoleon I. Those who assume French education to have been a model of Cartesian clarity in structure and operation during the middle of the nineteenth century will be undeceived here. The ideological cross-currents, the unlikely alliances, and the paradoxes abound. All shades of opinion seemed agreed that primary education should be moralistic and Christian for the sake of a stable society. Beyond that there is division on almost every other topic. Napoleon III showed little understanding of or interest in the issues. He wished simply to assure state control while avoiding the loss of clerical support; when there were choices to be made, he valued order over liberty; and the niggardly education budgets were the best indicators of his commitment.

*Un Ministre de l'Instruction Publique sous l'Empire autoritaire, Hippolyte Fortoul, 1851-1856* fills a gap noted in Anderson's bibliography. Fortoul is seen here as an impatient opportunist, whose early years in the liberal camp were spent seeking fame, first as a critic and novelist and then as a university professor of literature in the provinces. A political career beckoned in 1848, and he cultivated Prince Napoleon Bonaparte. His reward was the Ministry of Education from 1851 to his death in 1856. Fortoul's plans for the renewal of French education were limited by the parsimony of the government, the anticlericalism of the teachers in the lycées, and the classicism of the professors in the university. He hoped for a unified educational system which would be responsive to the needs of a strong and stable state. Yet the intellectual, spiritual, economic, or social interests of those who had their own intentions for the schools and the university meant that most of his programs were stillborn or short-lived. Fortoul left behind a reputation for a more rigidly centralized system of secondary education and for his political repression of the University. The authors succeed in showing the extraordinary difficulty an education minister would have had in pushing through the thickets of conflicting private and public ambitions during the authoritarian empire and the factors in Fortoul's temper and outlook which found him at his death a self-styled failure.

Anderson's book can become a point of departure for anyone who seeks a judicious and thorough survey of French education from 1848 to 1870; the other two works under review will have to be taken into account by anyone who expects to pursue further specialized study of the periods in question. All three works deserve serious study by the scholar who is not a specialist in the history of education.

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REED G. GEIGER. *The Anzin Coal Company, 1800-1833: Big Business in the Early Stages of the French Industrial Revolution*. Newark: University of Delaware Press; distrib. by Temple University Press, Philadelphia. 1975. Pp. xiii, 345. \$18.00.

Full credit is due Reed Geiger for further extending the thesis that the French Industrial Revolution was evolution. The Anzin Coal Company provides real proof of gradual growth during the 1800-1833 period. Under the leadership of the Desandrouin family and, after 1817, the Perier brothers, the highly profitable monopoly gradually introduced new methods to meet changing conditions and market demand.

The Anzin Company enjoyed a feudal monopoly on its lands in the area north of Valenciennes. A tariff helped to compete with Belgian coal. The market area expanded slowly, based on the river-canal transportation system. Although the company dominated the local and nearby markets, the distant ones, such as Paris, came later. Buyers complained about the monopolistic practices, but the company evidently tried to provide the proper types of coal to the particular industry.

The analysis of the company's operations is effectively expressed in chapters on technical development, geological and operational problems, costs, productivity, and profits, as well as the expansion of mineral concessions. The rich continental coal fields often yielded good-quality coal ranging from semianthracite through volatile bituminous. Thin and fractured seams, explosive gases, and excessive water led, however, to high-cost operations. In turn, this pushed the company to innovations, such as steam pumps and winding machines to raise water and coal from greater depths. Circumstances often forced the company, not always willingly, to divert profits to improvements, with the result that Anzin became a leader in ventilation systems, water control, and safety lamps in the ever-deepening shafts.

The maps are indispensable for locating mining areas, pits, and the river-canal systems for marketing coal. Footnotes, bibliography, and indexing are adequate.

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THOMAS D. BECK. *French Legislators, 1800-1834: A Study in Quantitative History*. Berkeley and Los Angeles: University of California Press. 1974. Pp. x, 202. \$13.75.

Thomas Beck's ambitious study of all the lower houses of the French legislatures from Napoleon's *Corps Législatif* to the Chamber of Deputies elected in 1834 quantifies and analyzes significant aspects

of the relationship between French politics and society during a historically crucial and historiographically controversial period. The Revolution of July 1830 is the "focal center" of the work. Although the quantitative analysis of the 2,862 members of the various Chambers and the attempt to correlate the attributes from which they were elected do not fulfill Beck's ambition to recapture the "true social reality of the period," they do illuminate salient issues in French political and social history.

The major attributes of the legislators include age, "social class" (based on the prerevolutionary definition of Estates), occupation, experience in government, party (Beck understands that contemporary "parties" were political factions rather than disciplined organizations), and percentage of vote obtained. The departmental characteristics are computed from a set of social, economic, and demographic variables that distinguish the more urbanized, wealthier, and "modernizing" departments from the more rural, poorer and "backward" departments.

The major burden of the argument is borne by relating the electoral results, in percentages of deputies returned for each political group, to the various characteristics of the deputies. For example, Beck's examination of Napoleon's housebroken legislators (for whom of course there were no party labels) confirms the view that Napoleon relied at first on what might be called a revolutionary bourgeoisie, but gradually turned to products of the imperial system and to elites with political and social roots in the Old Regime. The moderate royalist deputies who dominated the legislatures of 1816-1820 and 1827-1830 were characteristically, though not exclusively, employed in business and the professions, while the ultraroyalists, notably in the conservative legislature of 1824-1827, were dominated by "traditional elites"—those descended from the Second Estate and attached to the land. By 1834, as far as the electorate was concerned, "The Old France, with its traditional elite, was gone."

Put so baldly these observations may seem commonplace, but as they are worked out in Beck's painstaking analysis they do shed new light on issues that are by no means resolved in the current literature. The more sophisticated manipulation of the departmental data, however—Beck's social, economic and demographic variables—contributes less interesting results. The reader does learn that the political Left, at least in the elections of 1824 and 1827, drew its strength from "the more advanced and wealthy areas," and that the fate of the Bourbons might be read in their loss of support in the more progressive parts of the country. This suggestive observation is diluted by the fact that

there are no such correlations for the elections of 1819, 1820, 1831, and 1834. Beck concludes that the departments "voted in a random manner" in those elections. Presumably, he means that there was a random distribution of votes in relation to his variables. More refined explanations, based on a closer analysis of particular departments, are not in principle precluded, but are not contributed by Beck.

Some of the additional insights and generalizations drawn from Beck's imaginative manipulation of the political and social data are persuasive and illuminating. Others are shallow or debatable, and none can be adequately discussed in the space allotted here. One insight—"... it is obvious something fundamental changed because of the Revolution of July 1830," reassures the reader that realities available to the plain sense of contemporaries, but no longer self-evident to historians, might be recovered through techniques of systematic analysis.

ALAN B. SPITZER  
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CHRISTOPHER DUFFY. *Borodino and the War of 1812*. New York: Charles Scribner's Sons. 1973. Pp. 208. \$10.00.

RICHARD HUMBLE. *Napoleon's Peninsular Marshals: A Reassessment*. New York: Taplinger Publishing Company. 1975. Pp. 228. \$10.95.

Christopher Duffy's book presents a general account of Napoleon's invasion of Russia and a more specific analysis of the battle of Borodino and its consequence upon both the French and Russian armies. Although the narrative of the battle is necessarily complex as a result of the multiple battlefield operations, the author has produced well-organized, precise, and clear accounts of the diverse actions at the village of Borodino, at the Raevsky Redoubt, at the Bagration *flèches*, beyond the Kolocha River, etc., explaining the interrelationship and effects of each action upon the other.

The author introduces little new information regarding the battle since his study is based primarily on published Russian collections of eyewitness accounts, supplemented by a limited number of accounts from French participants. He does, however, present the English reader with valuable details and interpretations of the battle, which heretofore have been available only in the Russian language. Duffy has skillfully woven numerous eyewitness accounts, especially Russian, into his narrative to depict accurately the attitudes of the protagonists. The cavalry charges, the assaults along the Russian lines, the field hospitals, the

battlefield carnage, etc., are described with meticulous detail and give the reader a sense of involvement. The character sketches of the Russian generals are perceptive and accurate, and Kutuzov's inactivity at Borodino is carefully presented. The author seems to overemphasize, however, the abilities and contributions of General Barclay de Tolly, considering the latter's inept withdrawal and disorientation beyond Smolensk which would have resulted in disaster if Junot had been equal to the task. No doubt readers will be pleased with the valuable and provocative concluding chapter—an analytical discussion of Russian historiography and, more specifically, current Soviet scholarship on Borodino and the 1812 campaign.

Regarding emphasis, it is unfortunate the author did not devote more than two pages to the numerous and varied causes of the war and reduce the material in the three chapters and appendix pertaining to a discussion of the weapons, tactics, and general composition of the armies. Nevertheless, Duffy's book is a well-written general study of the battle of Borodino; it should serve as a good introduction for further work on the topic.

The second book under review is Richard Humble's reassessment of Napoleon's Peninsular marshals, which is, in fact, a general narrative of the entire Peninsular War. In addition to the biographical sketches of the sixteen marshals who served in the Peninsula, the author discusses each of their campaigns and achievements against the backdrop of Allied opposition. He concludes his study with an evaluation of the marshals and their contributions to the French cause. It is no surprise that Masséna is regarded as "far and away the best field commander ... in the Peninsula," followed by Soult, but it is indeed a shock to see that Ney, whose brilliant rear-guard actions saved Masséna's army during the retreat from Portugal, ranked low among the Peninsular marshals. Certainly Wellington, Picton, Napier, etc., had a different opinion of Ney.

The author describes the many personal, logistical, administrative, topographic, and strategic problems encountered by the French commanders in the Peninsula, and he re-evaluates their diverse and often contradictory operations against the Anglo-Portuguese and Spanish armies. Despite the vast scope of this topic and its many ramifications, Humble has written a clear, compact, and lively narrative of the war.

Although the prologue claims the book was written to provide "a look at the Peninsular War from the French side," it does not appear as though any of the French archival sources or many of the basic primary sources published in French or English have been consulted. Moreover, there is little evidence that the information in this volume cannot



be found in other more authoritative works. Occasionally, this study includes errors and misrepresentations in fact or emphasis. Humble's volume can, however, provide an interpretative overview of the Peninsular War and the commanders who served there.

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FRÉDÉRIC BLUCHE. *Le plébiscite des Cent-jours (avril-mai 1815)*. (Centre de Recherches d'Histoire et de Philologie de la IV<sup>e</sup> Section de l'École pratique des Hautes Études. Fifth Series, Hautes études médiévales et modernes, 21.) Geneva: Librairie Droz. 1974. Pp. xvi, 149.

Official but undated returns from this plebiscite recorded civilians' votes of 1,303,073 yes and 5,077 no. Frédéric Bluche's research enables him to give corrected totals of 1,308,842 yes and 5,145 no. His recount of all the votes in one-fifth of the departments reveals that the official returns understated their yes vote by .9 percent and their no vote by 6 percent; applying these corrective percentages to the other sixty-nine departments suggests that possibly even Bluche's corrected totals are understatements, and the real totals may have been about 1,315,000 yes and about 5,565 no. Clearly, however, there was not in 1815 anything like the massive fraud perpetrated in 1800 by the Ministry of the Interior and analyzed in 1972 by Claude Langlois (*Annales Historiques de la Révolution Française* [XLIV]).

Interpreting political options in any of the Napoleonic plebiscites is a delicate task. The majority response was always abstention. Bluche compares the plebiscites of 1800, 1802, and 1804 with this one, examines large-scale and small-scale geographic differences, with seven maps, and studies the attitudes of influential men—priests, notaries, and mayors. Illuminating though his discussion is, it is not exempt from criticism. For instance, a table (p. 108) gives the percentage of mayors voting yes and the percentage of eligible voters voting yes in twenty-seven departments. Bluche could have calculated the correlation coefficient ( $r = 0.434$ ) which puts his data in a clearer perspective.

There are still discoveries to be made in the Napoleonic plebiscites, but Bluche's is now the best study of any of them and the starting point for further research.

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FRANK RICHARDSON. *Napoleon's Death: An Inquest*. With a foreword by JAMES A. ROSS. London: William Kimber. 1975. Pp. 271. £4.95.

This book is an exercise in historical malpractice by a retired British Army surgeon. It is a sequel to his *Napoleon: Bisexual Emperor*, though not so blatantly designed to appeal to iconoclasts and the Gay Liberation Front. The medical opinions that follow have been validated by my friend, Dr. W. N. Adams Smith, Dean of the Medical School of the University of South Carolina.

General Frank Richardson makes a convincing case for Napoleon's having contracted amoebic hepatitis on Saint Helena. If readers accept the fact that he did, however, it remains to be proved that it killed him. Stomach cancer, which the author rejects, still seems the more probable cause of death. All retrospective diagnoses, as Richardson candidly admits, must be based on the findings of the eight physicians present at the autopsy, and the consensus was for cancer.

Nevertheless, if the author had confined himself to the causes of Napoleon's death, he would have had a fair little book of the genre he fancies. Instead, he has included introductory chapters which picture Napoleon as a Hitlerian ogre, selecting his evidence with all the objectivity of a Sir Walter Scott. Then in his epilogue he concludes that Napoleon is immortal. (The love-hate syndrome of a Chateaubriand?) Throughout, he reveals that he has read little on Napoleon published since World War II.

Richardson further spoils his work by rehashing the bisexual emperor business. Napoleon, he says, suffered in later life from pituitary dysplasia, and thus got fat, had unmasculine breasts, etc. *Ergo*, he was bisexual. Nonsense. Probably he did have the malady, but there is *no* evidence to show that it produces abnormal sexual appetites or behavior.

As a historian, Richardson is an irresponsible sensationalist; but it is hard to condemn him personally. He detests tyranny and despotism and places a high value on human life. He praises Wellington's magnanimity toward Napoleon and damns Blücher, who wanted him shot. Richardson is, withal, a man of principle and noble sentiments. A curious mixture, the general. Perhaps indeed there'll always be an England.

OWEN CONNELLY  
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ROBERT J. BEZUCHA. *The Lyon Uprising of 1834: Social and Political Conflict in the Early July Monarchy*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xv, 271. \$12.50.

This is a superb study of the most literate and autonomous community of workingmen in post-revolutionary France, examined at the point where

its social goals came into sharp conflict with the political liberalism, antirepublican fears, and mobilized interests of the Orleanist monarchy. The uprising of 1834 was, Robert Bezucha argues, the last instance of social radicalism within the worker community of Lyons. Its leadership, aroused by the threat that mutualist organizations would be destroyed by the new law on associations, and that a livelihood would be denied them, struck in April of 1834 as they had two months earlier. This time, however, the government was ready to terminate its ambiguous relationship with the social movement. In Lyons, Paris, and a few other centers, the organizations and personnel of the social and political opposition were crushed by massive arrests and a manipulated trial. The Orleanist government insisted that the uprising was manipulated by a republican conspiracy against the constitutional government of the July Monarchy. Neither the Orleanists nor the republicans had a direct interest in presenting the case of the *canuts* (silk weavers) in its own terms.

The radicalism of the Lyons silk workers had its origins under the Old Regime in the tension between silk merchants and weavers. The struggle took form in the demand for a fixed minimum rate for piece-work and adequate representation of the weavers in whatever councils were erected to regulate the industry. Neither demand was satisfied under the Old Regime or the Revolution, and both surfaced again in the grievances of the early 1830s. By then the *canuts*, representing one-quarter of the working force in Lyons, had their own tightly controlled neighborhoods, voluntary associations, newspapers, and community goals.

The silk workers, nonetheless, were politically fragmented—divided among Bonapartist, Carlist, and republican factions. In this condition, their own social efforts were overwhelmed by the larger struggle of Orleanist elites for political and social control of the urban centers. By 1846–48, when the social question was raised again, the *canuts*, weakened by technological changes within the industry and the decline of the international market for silk, no longer constituted the same community with the same demands. 1834 was a watershed in the social history of Lyons.

Within the area of its most obvious social and regional concerns, Bezucha's study clearly complements and merits an unchallenged place beside the work of Fernand Rude and Maurice Garden, who studied Lyons and the silk workers at other moments in their history. For historians concerned with social structure, it commands attention for its analysis of fiscal and census data, along with its use of administrative, arrest, and trial records. Beyond this, however, the author's didactic interest in social theory and comparative history makes

this work an indispensable volume for anyone broadly interested in the social and political processes of industrialization.

DANIEL P. RESNICK  
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SANFORD ELWITT. *The Making of the Third Republic: Class and Politics in France, 1868–1884*. Baton Rouge: Louisiana State University Press. 1975. Pp. xii, 329. \$17.50.

This book, the result of extensive reading in both the printed and manuscript sources, is a good example of present-day Marxist history which rejects pure economic and money-interest determinism. Sanford Elwitt's thesis is that republican bourgeois politicians founded the Third Republic, shaping it to conform to their class interests. In itself this is not an original conceptualization; it does not go beyond the age-old assertion that most of the revolutions and governments following them since 1789 were "bourgeois."

Where he more decidedly departs from traditional studies is in his perception of class conflict and in his use of local history to discover the areas of bourgeois republican strength. These chapters fit into his main thesis in that they reveal the great effort made by republicans to win over peasants and workers to the ideals of private property, individual freedom, social order, and "solidarity." This latter ideal was, in Elwitt's view, a myth used by bourgeois politicians to deaden class consciousness among workers. These chapters, although informative, are only partly satisfactory since they hardly go beyond electoral geography, failing to explain why republicans succeeded or failed in various locales.

More challenging is the section detailing the connections between republican politicians and industrialists. Elwitt finds the republican leadership coming mainly from a group of manufacturers. As such, they set out to defend industry from the financial oligarchy; they also sought to keep the working class in check lest the peasant and the petty producer, attracted by the oligarchy's moderation, swing to the right. By the late 1870s and early 1880s these republicans had not only achieved their main goal, they had founded a school system to inculcate the young in middle-class ideals, and they had expanded the empire to promote the economic interests of their class. They had transformed France into a bourgeois republic.

But now, what is a bourgeois republic? This expression, often used but rarely explained, is not sufficiently clarified by Elwitt. His idea of the Third Republic seems to identify it chiefly with industrial capitalists. Yet, he insists, they were

only part of the entire bourgeois class which comprised wealthy financiers as well as farmers and "petty producers" such as tailors, carpenters, shoe-makers, and corner retailers. Such a bourgeoisie in the 1870s would have included more than three-fourths of the active population. A bourgeois republic, therefore, was nearly France itself, save for wage workers. And the latter hardly emerge in the book.

For this reason the conflict involved in shaping the Republic was an "intra-class" affair, not one between different classes. Industrialists and their political representatives, supported by capitalist farmers, opposed financiers over issues such as railroads and secular education; over other issues such as tariffs they were in conflict with farmers and merchants. The only real issue on which they could all unite was expansion of the empire. Elwitt's insistence on unifying such diverse social and economic strata into one class has disadvantages. It has blinded him to the variety of republican movements after the *seize Mai* crisis of 1877, and his views on the Radical followers of Clemenceau are simply wrong. These weaknesses notwithstanding, his book is both challenging and an important contribution.

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J. NÉRÉ. *The Foreign Policy of France from 1914 to 1945*. (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1975. Pp. ix, 366. \$24.95.

KALERVO HOVI. *Cordon sanitaire or barrière de l'est: The Emergence of the New French Eastern European Alliance Policy, 1917-1919*. (Turun Yliopiston Julkaisuja; Annales Universitatis Turkuensis. Series B, volume 135.) Turku: Turun Yliopisto. 1975. Pp. 244.

Jacques Néré's book, which emphasizes the period from 1919 to 1940, presents a workmanlike analysis of the problems France faced, some of which developed initially when the United States refused to ratify the Treaty of Versailles and the Treaty of Guarantee and when differences arose between Britain and France over German reparations and disarmament. Although he presents a French point of view, Néré indicates the errors of French ways, some committed by diplomats at the Disarmament Conference and in the Ethiopian crisis, others by military men who were too attached to defensive principles based on obsolete concepts, as during the Rhineland crisis, and yet were too rash in moving, without proper protection, into Belgium in 1940. To him, French commitments in Eastern Europe are understandable, but he de-

plores both the blindness of Poland in keeping up conflicts with all of its neighbors, thus making an Eastern Locarno impossible, and the refusal of the Czechs to cooperate with Italy in solving Danubian problems, thereby making a Franco-Italian rapprochement difficult. He regards the Ethiopian crisis as the first confrontation in France between supporters of collective security and those favoring an alliance with Italy. He deprecates putting the blame on Britain for French inaction in March 1936; since the French public was essentially pacifist, and since the military men never intended to cross the German frontier, many in France regarded the reoccupation as of little consequence.

Though the early part of Néré's study is based on memoirs and the major monographic literature, he uses systematically the volumes in *Documents Diplomatiques Français* (1932-37) available when he wrote his book. The appendix of fifty documents, twenty-six of them from D.D.F., will be useful to students who lack access to a first-rate library, or are unable to read French.

Kalervo Hovi's research for his dissertation has covered extensive files in the archives at the Quai d'Orsay and the Public Record Office, in Brussels and Bonn, as well as in Vienna, Copenhagen, and Stockholm. He carefully explores the development of French policy in Eastern Europe from October 1917, when a very moderate view of the Bolshevik Revolution prevailed, until the autumn of 1919. During most of this period France was more concerned about creating a barrier against Germany than preventing the spread of Bolshevism. Although French policies were grounded on cooperation with her Western Allies, decisions about Eastern Europe were largely taken independently. France disliked the Bolsheviks initially because they had destroyed the Franco-Russian alliance and were thereby benefiting Germany; French intervention in southern Russia was undertaken partly to secure French economic interests, but more to eliminate German influence. France disliked British policy in the Baltic, which, on a short-term basis, countenanced the use of German troops to repel Bolshevism; France regarded the former as the greater danger. French aims in establishing a *cordon sanitaire* were more tactical and short-term, as compared with her anti-German stance, which was seen as a long-term necessity. France's whole policy toward the emerging nationalist movements was centered on encouraging them to make alliances with France or among themselves to counterbalance Germany's "structural hegemony" and prevent her further eastward expansion. Poland, and to a lesser degree, Czechoslovakia and Rumania were central to all French plans, but the Baltic states and Ukraine were intended to serve as a secondary line of defense.

France's attitude toward Bela Kun in 1919 was reserved and was dictated by strategic considerations that arose from fears that in opposing him, Hungary's neighbors would be deflected from carrying out the role assigned to them in the *barrière de l'est* system. Hovi's balancing of the various parts of French policy is convincing.

Together these two books add in both substance and interpretation to the historiography on French foreign policy.

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D. BRUCE MARSHALL. *The French Colonial Myth and Constitution-Making in the Fourth Republic*. New Haven: Yale University Press. 1973. Pp. xii, 363. \$12.50.

Historians who may have looked only at the title of D. Bruce Marshall's book and shunned it as yet another attempt to recapture ephemeral mythology or another dull essay into endless French constitutional conflict should know that the title is misleading. The volume is a lucid and enlightening contribution to French history—colonial, political, intellectual as well as constitutional. The author's purpose is to show how the leaders of the early Fourth Republic tried to hold together the empire threatened by the rising forces of native nationalism and to explain why their efforts failed. He focuses on the two years from the Liberation to the adoption of the new constitution in 1946, but his approach is historical, and his book is a sound introduction to French colonial history, especially the ideologies and policies underlying it in the nineteenth and twentieth centuries.

Marshall defines "the colonial myth" as the belief that France and her colonies were bound together by an indissoluble link based on shared universal ideals of the Rights of Man, that France's *grandeur* depended on retention of the colonies, and that France's responsibilities to raise native peoples to French levels of civilization required maintenance of the Metropole's predominance in the governing of the colonies. The acceptance of this myth by Frenchmen of all parties blinded them to the realities of native nationalisms and, more than any other single influence, caused the delegates to the second constitutional assembly to declare their commitment to liberty, equality, and fraternity for all peoples of the French Union. But the myth also caused them to write into the constitution of the Union both inequality of states (the Metropole remained dominant) and inequality of peoples (metropolitan and colonial French enjoyed more rights than native peoples). The ideals of the myth encouraged aspi-

ration to self-government; the institutions of the Union denied it. This combination assured that native leaders would look to other myths—African socialism and Arab unity—and resort to violence. Perhaps no set of institutions could have held together a nineteenth-century colonial empire in the mid-twentieth century, but the French commitment to their myth made their divestiture of empire particularly bitter and bloody.

Marshall's thesis on the influence of the myth is undergirded by firm knowledge of French political and colonial history, and it is convincingly argued. He does, however, give due weight to other influences on the decisions of the constituent assembly—party politics, De Gaulle's ambitions, the Communist party's complex maneuvers, and the war in Indo-China.

Readers will regret that Marshall did not continue his study to the final agony of empire in the 1960s and consider the fate of the colonial myth as the nation adjusted ideologically to the end of empire. But that would be another book.

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PEDRO DE MEDINA. *A Navigator's Universe: The Libro de Cosmographia of 1538*. Translated and with an introduction by URSULA LAMB. (Studies in the History of Discoveries.) Chicago: University of Chicago Press for the Newberry Library. 1972. Pp. 224. \$18.50.

Pedro de Medina is best known as the author of the *Arte de Navegar*, a textbook on navigation first published in 1545, with many subsequent editions in Spanish and foreign languages. He wrote also on various other subjects, but his chief interest lay in cosmography and its relevance to nautical practice. This was in fact the subject matter of the manuscript "Libro de cosmographia," which Medina submitted to the king of Spain in 1538, gaining thereby the right to produce charts and instruments under the authority of the Casa de la Contratación in Seville and, eventually, to examine pilots. A copy of this work was discovered in the Bodleian Library in 1959 and is now published for the first time. The handsome facsimile is edited by Ursula Lamb, who contributes an erudite account of Medina's life and writings, a translation of the text, and some notes thereon. The volume is one of the monograph series published under the auspices of the Society for the History of Discoveries.

In the *Libro*, cosmography is presented by way of question and answer, the questions being put by a graduate (*licenciado*) and a pilot and answered by a cosmographer. The result has the tone as well as the content of an elementary manual of instruc-

tion; indeed Medina makes it plain in the dedication that he "wished to aid those who want to know something of that science, and in particular to advise sailors upon the seas." Unlike the *Arte*, however, the present work was presumably never used for teaching, and though doubtless it reflects to some extent what was taught by Medina himself and more generally by other instructors at the Casa at that time, it is largely a theoretical work, lacking most of the features of a typical navigational manual. As a theoretical work, it is of special interest because it attempts to set out in very simple language, and with reference to everyday experience, the collection of concepts a well-educated navigator might reasonably be expected to know. It can therefore tell the reader much about the state of contemporary cosmographical thinking that could not be learned from, say, Copernicus's *De Revolutionibus*, which appeared in 1543. The editor has little to say about this aspect of the work, but one must be grateful to her for placing so interesting a document on view and for describing so well the circumstances in which it was written.

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HEINZ GOERKE. *Linnaeus*. Translated from the German by DENVER LINDLEY. New York: Charles Scribner's Sons. 1973. Pp. xi, 178. \$9.95.

Among eighteenth-century scientists none had so great an impact on the field of natural history as the Swede, Carl Linnaeus (1717-78). Book-length biographies of him number more than seventy. Articles about him and his contributions number in excess of 4,000. In this small volume Heinz Goerke provides a readable overview of the man and the scientist: a work written by a physician fluent in Swedish, intimate with primary source materials, and a recognized authority on European medical history.

The work presents the many-faceted Linnaeus: teacher, botanist, zoologist, physician, pharmacologist, and sociologist. The impact of his major publications is analyzed and evaluated. It offers one of the best appraisals of Linnaeus the physician. Linnaeus' contributions to diet as a factor for good health, his classification of diseases, and his studies of disease diagnosis and therapy are well developed. The student of Linnaeus will welcome the abundant citation of reference notes, the accurate chronology of the man's life, and the annotated list of supportive biographical works.

It is regrettable that the volume is reduced substantially from the original German edition (Stuttgart, 1966), and that some of Goerke's erudition as a writer is lost in translation. The illustrations (about half of the number in the German edition)

are reproduced so poorly as to bring discredit to the publisher. The originals which they represent are not out of focus.

The brevity of the biography precluded inclusion of much available anecdotal material so essential to add life to the subject. For this, one must consult the cited works by Blunt, Gourlie, Hagberg, and Jackson. This volume is to be consulted for its accuracy and balance, for its careful analyses of the man and his works, and as an introduction to reliable source materials.

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RICHARD MARIUS. *Luther*. Philadelphia: J. B. Lippincott Company. 1974. Pp. 269. \$8.95.

Yet another Luther study appears in the already saturated English market. Why was this book written? Apparently in order to demythologize the image of a hero and bring out the sharp edges—the provincial intolerance, the claim to infallibility as Bible interpreter—in short, the less amiable aspects of the Reformer. Moreover, Luther has "no word to speak to our time" (p. 11). Why then concern oneself with Luther at all? "We study him because he is studied"—a great "bulk of a name heaving itself out of our past with the insistent demand that we pay attention to him" (p. 254). Although there is nothing substantially new in the book, it provides a readable narrative with a penetrating, sometimes superb, if a bit lengthy, analysis of certain key writings. Marius is basically sympathetic to Luther. He dates Luther's insight of justification by faith with Bizer at 1518, thus disagreeing with a majority of Luther scholars who date it to 1513-16 (though this issue has been blurred almost hopelessly by semantics). Like Lortz, Marius points to the almost universal Catholic misunderstanding of Luther's "Faith" as dead faith. On the famous issue of the "nailing" of the Ninety-five Theses he sides with a pupil of Lortz, Iserloh, who rightly doubts the fact.

Criticisms, *inter alia*: Only the literal sense was the basis of dogma in the Middle Ages (Thomas and others). Already Gratian preferred truth to custom. Oberman has rightly doubted that there was Catholic "orthodoxy" in Luther's time, given the many contradictory concepts of justification (Thomist predestinarianism and Nominalist Pelagianism), but also the conflicting theories on infallibility, including the denial of any institutional infallibility by "orthodox" writers. Luther's position at Leipzig in 1519 was objectively nothing novel. The thesis that Luther's anti-Semitic tracts of 1543, contrasting with his tolerant attitude in the 1520s, were a direct consequence of his use of the Bible seems debatable in view of many other



inconsistencies, some even at the core of his message. Could not Luther's propensity to write not systematically but situationally be responsible?

Apparently the author's intellectual roots lie in the Enlightenment, as his appeal to Holbach in connection with the alternative "Auschwitz or Jehovah" or the reference to the Bible as an "absurd collection of folklore and phantasy" shows (ch. 28). He stops, however, where the task should begin. Respectable scholars are trying to determine the historical truth behind that "folklore." The question of Christ is essential to any attempt to make Luther relevant today. Ebeling's respective efforts are dismissed without any serious discussion. Is Marius' attitude reminiscent of the intolerance of the Enlightenment toward other than enlightened views? His book suffers from such one-sidedness. Nevertheless, it is a sobering corrective of an equally one-sided glorification of Luther.

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MARTIN KITCHEN. *A Military History of Germany: From the Eighteenth Century to the Present Day*. Bloomington: Indiana University Press. 1975. Pp. 384. \$12.50.

The title of Martin Kitchen's new book gives little inkling of its nature and scope. It is not a history of operations or of the evolution of tactical and strategic doctrine. Like Hintze and Delbrück, Kitchen is interested principally in the intimate relationship that exists between a nation's economic, social, and political arrangements and its military institutions and policies. His success in demonstrating this—he shows, for instance, how the failure to reform the Frederician military system before 1806 or to expand the army as the General Staff wished in 1912–13 resulted from fear of the political and social changes that would follow—gives this book greater interest to the general historian than to the military specialist.

This is not to say that the latter will not find good things in it. Kitchen's accounts of the operations against France in 1870 and of the two world wars are concise, but filled with interesting detail; and his analyses of Frederician warfare, of Scharnhorst's reforms, of Helmuth von Moltke's political and military philosophy, of the deficiencies of German strategy in the age of Wilhelm II, and of Hitler's talents as *Feldherr* are shrewd and illuminating. But the most satisfying and original parts of the book are those that deal with the army's role in society: its conflict and accommodation with middle-class liberalism in the nineteenth century; the variety of weapons that it em-

ployed in its fruitless attempt to destroy the militancy of the working class (a subject treated with authority in Kitchen's book on the officer corps in Wilhelmine Germany); its ascendancy in state policy before and during the First World War and the tragic results that flowed from this; the baleful political activities of its leaders Seeckt and Schleicher during the Weimar period; and its debauching and destruction by Hitler, for whose rise it was largely responsible.

Kitchen's "Aftermath" chapter, which deals with the military history of the Federal Republic, is the least successful part of his book. He is convinced that German rearmament was a mistake, caused by the need of the Western military-industrial complex to sustain itself, and that the Bundeswehr's "strategic thinking is based on an anachronistic and manichean world view, [its] structure and armaments are inappropriate for its professed role, [its] ideological basis is suspect, and [its] functional role in post-war German society is open to severe criticism" (pp. 352–53). The arguments that he uses to support these views are not, to this reader, persuasive and seem to be based on questionable assumptions about Soviet intentions, inaccurate estimates of the relation between the strength of the NATO and Warsaw Pact powers, and unjustified intimations concerning the threat posed by the Bundeswehr to West German democracy. The best that can be said of this essay is that it is vigorous and provocative and does not detract from the great merits of the book as a whole.

GORDON A. CRAIG  
Stanford University

WOLFGANG HARDTWIG. *Geschichtsschreibung zwischen Alteuropa und moderner Welt: Jacob Burckhardt in seiner Zeit*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, Schrift 11.) Göttingen: Vandenhoeck & Ruprecht. 1974. Pp. 405.

This significant monograph sets out to discover why Burckhardt's credit as critic of his times stands higher today than that of Ranke, Treitschke, or any other nineteenth-century German historian. The answer is found convincingly in Burckhardt's singular, "Old European" point of view. The author, however, could well have chosen as title, "Between Historicism and Humanism," for he devotes disproportionate space (four of the book's five sections) to basic questions of history's meaning and value. With remarkable analytical precision he shows that Burckhardt accepts the full secularity and historicity of human experience, while recognizing man's cultural continuity, especially in art, as a source of eternally

valid self-knowledge. For Burckhardt, art offers a sacred revelation of man's inwardness. A second creation, transcending history's ordinary limitations, art serves in all ages to purify and elevate man. The author contends, however, that this is not estheticism. Burckhardt's ultimate norm is ethical: *humanitas* or self-determination.

When the author finally turns to the central issue, Burckhardt's perspective on the crisis of his times, he fails to link it securely to the preceding analysis. Nonetheless, he demonstrates that Burckhardt's perspicacity was due to "Old European" political outlook, drawn from the patriarchal, socio-political order of Basel and, above all, the "classical" or "Aristotelian" tradition of natural law. These sources combined to form in Burckhardt an "anachronistic and idealistic" image of a community of citizens conscious of their duty as well as their rights, which he juxtaposed to nineteenth-century reality.

Although filled with insights, this part of the monograph seems less complete than those dealing with historicism. The first topic sentence on page 304, for example, is garbled. Moreover, the author argues ineffectively (p. 290) for "essential development," even "radical changes," in the historian's life and thought. The example given, Burckhardt's liberalism at Berlin, 1841-44, reflects a romantic attachment to his friends' liberated style of life more than to their political outlook. The author a few pages later weakens his argument by citing Burckhardt's critique of French politics in the Bourgeois Monarchy, 1843, anticipating most of the points he would make in the 1870s.

Despite its diffused focus and abbreviated treatment of Burckhardt's political outlook, the monograph is the most illuminating single-volume interpretation in at least a generation of the great Swiss historian. Hardtwig's "hermeneutical" method copes expertly with the baffling antinomies of Burckhardt's thought.

CHARLES H. O'BRIEN  
Western Illinois University

JOHANN JACOBY. *Briefwechsel, 1816-1849*. Edited with introduction by EDMUND SILBERNER. (Veröffentlichungen des Instituts für Sozialgeschichte Braunschweig.) Hanover: Fackelträger Verlag. 1974. Pp. 668.

As one of the leaders of German democracy in the nineteenth century, the Königsberg physician, Johann Jacoby, had to be content with a secondary role in politics as well as in historiography. Publication of his correspondence, of which this is the first of two volumes, is long overdue.

In an introductory essay, Edmund Silberner describes the status of sources on Jacoby as "not at

all favorable," since his legacy has been all but lost because of the war. Most of the letters available to Silberner were copies, not originals. The core of the present edition (material collected by Gustav Mayer and donated to the Jewish National and University Library, Jerusalem) even consists of copies of letter drafts, not actual letters. Given these difficulties, Silberner has produced an impressive edition of documents, most of them previously unpublished.

The collection includes letters to and from Jacoby, letters from his circle of friends, public declarations, poems, and other documents in which Jacoby plays a part. The material's chronological distribution is necessarily uneven. Few documents exist on Jacoby's youth. The bulk of the material covers the revolutionary period. Of the 645 documents assembled in this volume, 167 date from 1848, which Jacoby himself regarded as the zenith of his political life.

Editorial annotations indicate the location of each document and, generally, deviations among various sources. Yet, a comparison of the letters in this edition with those at the *Bundesarchiv*, Frankfurt, reveals quite a few—though insubstantial—discrepancies, mostly limited to interpunctuation and underlining. Other Jacoby material available to the reviewer showed no such discrepancies. Silberner demonstrates in his footnotes patient attention to the minutiae of editing. Extensive biographical explanations deserve special mention. Bibliographical notes are kept deliberately at a bare minimum. Copious annotations pertaining to Jewish history reveal the editor's interest and expertise in this field.

While working on this edition, Silberner published several lengthy biographical articles on Jacoby based on the letters and other unpublished material, some of which—like Lotte Esau's notes on the Jacoby diary—this reviewer had hoped to find included in the present volume. Thus, the edition of letters itself reveals no new startling information about Jacoby. It does, however, provide valuable additional information on a wide variety of problems pertaining to the democratic and liberal opposition of the prerevolutionary and the revolutionary years. The book is without question an important, well-done addition to our printed sources on nineteenth-century German history.

ARNOLD SCHUETZ  
Virginia Commonwealth University

ALBIN GLADEN. *Geschichte der Sozialpolitik in Deutschland: Eine Analyse ihrer Bedingungen, Formen, Zielsetzungen und Auswirkungen*. (Wissenschaftliche Paperbacks, 5; Sozial- und Wirtschafts-geschichte.)

Wiesbaden: Franz Steiner Verlag GmbH. 1974. Pp. x, 207.

One of a series of broad syntheses intended for students of German social and economic history, this volume may be useful to professional historians who want a brief introduction to both standard and recent works on the development of German social policy. The author takes pains to relate this development to "structural" changes in economic and social life and, where his sources are adequate, does so with admirable clarity. His first chapter, dealing with the origins of social policy, is limited to Prussia in the early nineteenth century. Subsequent chapters are uneven. The one on the Hohenzollern Empire is best; those on the Weimar and Nazi periods are thin and disappointing. The book, nevertheless, succeeds as a whole in demonstrating that in spite of the revolutions and political catastrophes which have struck modern Germany, the country's social policy, with its paternalistic tradition, its emphasis on self-help, and its concern for social welfare in the interest of national strength, has a continuity of its own. The book's major defect is its superficial treatment of the results of social welfare measures. The fault is hardly the author's for it is clear from his text and his bibliography that German historians and social scientists have not produced sophisticated studies of this subject comparable to those in which British scholars have examined the achievements and shortcomings of their welfare state.

DONALD G. ROHR  
Brown University

URIEL TAL. *Christians and Jews in Germany: Religion, Politics, and Ideology in the Second Reich, 1870-1914*. Translated by NOAH JONATHAN JACOBS. Ithaca: Cornell University Press. 1975. Pp. 359. \$19.50.

This is an important book for an understanding of both German history and Jewish history in the modern period. The study of the roots of twentieth-century anti-Semitism in Germany has hitherto concentrated on the *Völkisch* movement, with some attention to the economic and social aspects of anti-Semitism. This study, dealing with a broader range of intellectual attitudes, examines how all major parties (except the Socialists) in the new German state looked upon the efforts of Jews to find a place within it. The majority of German Jews relied on the deceptively simple formula of integration and equality to enable them to be both Germans and Jews. This, the author argues, was unworkable, since those groups which welcomed Jews as individuals (the Liberals) demanded that they renounce Jewish particularism, while those who were willing to accord a certain status at least

to Orthodox Judaism (the Conservatives) did so on the condition that it remain outside the Christian state. Liberal Judaism—and that meant most German Jews—had few friends either among Catholics, who identified it with their enemies in the *Kulturkampf* as well as with a variety of menacing aspects of modern life, or among Liberal Protestants. The latter, though opposed to anti-Semitism, stressed the ethical superiority of Christianity over Judaism; the surprisingly vigorous Jewish attempts to show that the reverse was true were greatly resented.

Christian anti-Semitism was paralleled by the emergence of a more intense and more explicitly racist version with overtly anti-Christian elements as well. To some degree the greater responsibility of the former kept the latter from becoming a mass movement, at least before 1914. It is the great merit of this book that this complex of attitudes is seen not only in terms of the "Jewish Question," but as part of the general problem of German identity and self-definition. In this context the importance of Jewish-Christian relationships for the history of Wilhelmine Germany is made clear. The impressive erudition, the richness of detail, and the lucid style (rendered in a singularly felicitous translation from Hebrew) all contribute to the significance of the book. It will, perhaps, be a difficult work for those not overly familiar with the historical setting and background, both German and Jewish. But then this is not a book for the specialist only, and whatever effort is required to grasp its complexities and subtleties will be well worth the trouble for a wide range of readers.

SOLON BEINFELD  
Washington University

ROBERT GELLATELY. *The Politics of Economic Despair: Shopkeepers and German Politics, 1890-1914*. (Sage Studies in 20th Century History, volume 1.) Beverly Hills: Sage Publications. 1974. Pp. xvi, 317. \$15.00.

The social and political evolution of the German *Mittelstand* has recently been the subject of several detailed studies. Robert Gellately has added a well-researched account of the social circumstances and political mobilization of German shopkeepers before 1914. An epilogue traces their further radicalization during the Weimar era and links this book to recent regional histories of the early Nazi party.

Gellately warns against treating the *Mittelstand* as a single social entity. Whereas artisans and handicraftsmen suffered greatly from the cyclical economic downswings of the 1880s, retailers mostly enjoyed favorable economic conditions and thrived on cheap factory-produced consumer goods which

threatened the very livelihood of craftsmen. Transportation improvements, rapid urbanization, and increases in workers' disposable incomes greatly expanded the volume of retail trade and encouraged the proliferation of small retail outlets. In the 1890s, however, while the overall economy experienced an upswing, small shopkeepers faced severe competition from each other and from larger department and chain stores, consumer cooperatives, and installment purchase and mail-order firms. More effective organization of shop employees, together with government insurance and other legislation, also squeezed the profits of the small independent retailer.

Shopkeepers defended themselves against these threats. They formed local purchasing cooperatives and discount unions, called for boycotts of factories supplying consumer coops, demanded the establishment of independent chambers of retailers like the Prussian *Handwerkskammern*, and founded regional and national organizations. After 1900, especially, more overtly political organizations tried to mobilize support for legislative protection against "unfair competition." But no single organization managed to unite all shopkeepers. Local evidence from Hamburg reveals the great diversity of political and social views within their ranks. Gellately gives an interesting account of their divisions when Conservative finance proposals caused the dissolution of the Bülow bloc. He also demonstrates the importance of the Saxon organization, led by Theodor Fritsch and others, in steering most shopkeepers into the right-wing "Cartel of Productive Classes" in 1913. Sworn enemies of social democracy, most retailers also believed that their best hope to obtain greater social protection lay in supporting the efforts of big industry and agriculture to preserve the status quo. World War I and the early Weimar years further politicized and radicalized shopkeepers, making them vulnerable to extremists like Hitler.

Although an important contribution, this carefully researched book leaves a few gaps to be filled. Little evidence is given of the impoverishment of retailers or of their social mobility; no distinction is made between the impact of economic fluctuations upon larger and smaller retailers. While, as Gellately shows, anti-Semitism was rife among shopkeepers, no information is given about the numbers and role of Jews in retailing or the relationship between economically motivated anti-Semitism and the biological racism espoused by a man like Fritsch. Finally, the book assumes a fairly simple relationship between economic "despair" and political mobilization. Though convincing for the Weimar period a lot more evidence of retailers' incomes and bankruptcies (in addition to their claims of distress when seeking protective legisla-

tion) is needed to prove the connection for the pre-1914 era. The 1890s saw a veritable mania for political and social organization in Germany, and retailers' activities should be set within this context. Their growing group solidarity might perhaps be viewed as another indication of the emergence after 1895 of an "organized capitalism" in Germany, where traditions of state protection and customary notions cut across free-market ideas in a complicated way. More evidence is needed to show that economic despair was the chief motivating factor in the organizational efforts of shopkeepers before 1914.

GEOFFREY G. FIELD  
State University of New York,  
College at Purchase

SUSANNE MILLER. *Burgfrieden und Klassenkampf: Die deutsche Sozialdemokratie im Ersten Weltkrieg*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 53.) Düsseldorf: Droste Verlag. 1974. Pp. 440. DM 72.

This useful book describes the development of German Social Democratic policy from the war credits vote in August 1914, to the eve of Germany's military collapse in the fall of 1918. The growth of opposition within the party against the support of the war effort and the resulting split between Majority Socialists and Independent Socialists is the main theme. Most available printed sources and a great many manuscripts and oral testimonies are used. For this reason alone, Miller has made a great contribution to the history of socialism; her book will serve as a guide to further research. Although she apparently has more sympathy for the Independents than for the party majority, her judgment is as well-balanced as one can expect of any historian writing on a controversial subject. In contrast to other writers, she fortunately gives much space to the struggle of the Majority Socialists against annexationism and to their support of the Reichstag peace resolution of July 1917, although in this reviewer's opinion she still underrates the positive significance of that resolution and of the cooperation of the Majority Socialists with liberals and Catholics in the *Interfraktionelle Ausschuss*.

On some points, however, the reader might wish Miller's analysis had gone deeper. She might have explained the fundamental, as distinguished from the mere tactical, differences between the pacifist core of the Independent Socialists and the left radicals—between such men as Hugo Haase on the one hand and Karl Liebknecht and Julian Borchardt on the other. Miller speaks extensively of the differences in the attitude of the two groups to the issues of the day, but except for a few brief



hints the reader does not learn the nature of the philosophical antagonism—that Haase and his friends rejected violence in principle and that the people around Liebknecht and the other radical splinter groups asserted the legitimacy of revolutionary violence, leading to dictatorship. Yet it was this difference which eventually caused the Haase wing to rejoin the Majority Socialists and the other to form the Communist party.

In many ways German party politics during World War I was the prelude to the Weimar Republic, and the actions of parties during the war can be fully evaluated only in light of their consequences during the Weimar period. Susanne Miller announces in her *Nachwort* that she intends to write a history of German Social Democracy in the first postwar years. Aside from the valuable insights the reader will probably gain from this sequel, work on the continuation project may lead the author to a revision of her rather adverse judgment on the merits of the collaboration of the Majority Socialists with liberals and Catholics during the second part of the war. Without the forging of these links in 1917, the Weimar Republic would have lacked a political foundation; specifically, the *Erfüllungspolitik* under Chancellor Wirth, which was supported by both the Independents and the Majority Socialists, would probably never have materialized; and without the rapprochement of the two social democratic parties during the Wirth episode, their unification would hardly have been feasible.

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Berkeley

ALFRED D. LOW. *The Anschluss Movement, 1918-1919 and the Paris Peace Conference*. (Memoirs of the American Philosophical Society, volume 103.) Philadelphia: the Society. 1974. Pp. xiii, 495. \$8.00.

In the past, historians of the Austro-German *Anschluss* movement have concentrated their efforts on the events of the late 1930s and on Nazi Germany's annexation of Austria. Alfred D. Low examines fully and in great detail what might be called the "background to the *Anschluss*," namely the events of the year following the November armistice in 1918. During the first year after the war the waves of a reawakened *grossdeutsch* longing played a major role in the politics of rump Austria—German Austria—and, to a somewhat lesser degree, in the politics of Germany. The Germans, particularly Austrian Germans, took a naive view of Wilson's concept of national self-determination by believing that it would overrule all economic and strategic considerations in the postwar territorial settlements. This German concentration

on the possibility of an Austro-German merger was, as Low points out, an exercise in futility and national frustration for the Germans of both states, particularly for the Austrians. This frustration might have been avoided, along with the resultant irredentism, had the Allies spelled out clearly and quickly the reasons for their determined opposition to this national merger, which, after all, would have amounted to compensation for the territorial losses Germany was to suffer elsewhere. In retrospect, it seems quite clear that the ultimate treaty prohibition of the union was a foregone conclusion, but the author does not as clearly demonstrate that Allied policy was not united enough, nor were the Allies sure enough of their own position and strength to have issued an early public proclamation of their opposition.

Low presents a thoroughly balanced study of this topic, though an introductory chapter tracing the history of the *grossdeutsch* idea during the nineteenth century and the developments of World War I would have provided a clearer setting for the issues that arose after the collapse of the two German monarchies in late 1918. A more detailed examination of German and Austrian newspapers for the 1918-19 period would have further demonstrated the shifting fortunes of the *Anschluss* movement in public opinion. Such additions would have provided support for, but would not have altered, Low's conclusions.

Low brings to the study of this topic considerable background in the history of Central and Eastern Europe and of the history of the succession states in particular. The book under review will soon appear in a German version.

FREDERICK DUMIN  
Washington State University

MODRIS EKSTEINS. *The Limits of Reason: The German Democratic Press and the Collapse of Weimar Democracy*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xi, 337. \$22.50.

Modris Eksteins' book, *The Limits of Reason*, traces the decline and collapse of Weimar Germany not, as has often been done from the viewpoint of politicians and parties, but through the eyes of Germany's prestigious and articulate liberal-democratic press. The work focuses on the history of the three great publishing houses of Sonnemann, Ullstein, and Mosse and their respective newspapers—the venerable and scholarly *Frankfurter Zeitung*, the *Berliner Morgenpost*, *Vossische Zeitung*, and *Berliner Tageblatt*, among others. The family history of these publishing empires, from their inception in pre-imperial Germany to their decline in the Weimar Republic and their death under Hitler, would, in itself, form an engrossing tale. The au-



thor has provided scholars with much new information on the Ullstein family quarrel during the 1920s.

The family fortunes of the Sonnemanns, Ullsteins, and Mosses are only incidental, however, to the main theme of the book. This is a thoughtful and thought-provoking study of German intellectual and political liberalism in the empire, the revolution of 1918–19, and the Weimar Republic. Because of the nature of the German constitutional system of 1871 and the failure of the German liberal revolution of 1848–49, the liberal press in Germany, more so than in Western Europe, consciously cast itself in the role of spearhead and propagator of liberal-democratic reform. This remained the common creed of the great liberal publishing houses of Germany, reaching its noblest definition in Sonnemann's testament of 1909. The heirs of the Frankfurter *Zeitung*, he said, were to remain "politically liberal in social-political matters, always . . . friendly to reform, always inclined to support the economically weak. . . ." The author gives an intriguing account of the union and ultimate divorce of intellectual liberalism, as represented by the metropolitan liberal press, and political liberalism, as organized in the liberal parties. Perhaps the most challenging portion of the work is the question of liberalism's own responsibility in the political disaster of the republic. Impeccable in scholarship and often absorbing in its narrative, *The Limits of Reason* stands out as an important contribution to the understanding of twentieth-century Germany history.

ALEXANDER RUDHART  
Villanova University

THEODOR SCHIEDER. *Hermann Rauschnings "Gespräche mit Hitler" als Geschichtsquelle.* (Rheinisch-Westfälische Akademie der Wissenschaften. Geisteswissenschaften: Vorträge, G-178.) Opladen: Westdeutscher Verlag. 1972. Pp. 91. DM 11.70.

Hermann Rauschnig, a German nationalist turned Nazi (1931), served as president of the free city of Danzig in 1933–34. Because of the city's importance, Rauschnig was allowed to participate in a number of discussions with Hitler in the early 1930s. Subsequently disillusioned with the Nazis and their political squabbling, he broke with Hitler in 1934 and went into exile. After writing a theoretical denunciation of Nazism (*The Revolution of Nihilism*), he published an account of his conversations with the Führer (*Gespräche mit Hitler*) in 1939–40. A best seller in many languages, the *Gespräche* was for long the only volume purporting to present the essence of Nazism in the Führer's own words. In the postwar period, however, as other and more readily authenticated expressions of Hit-

ler's ideas became available, historians raised serious questions about the validity of Rauschnig's account.

Theodor Schieder has now attempted to determine the reliability of Rauschnig's *Gespräche* by searching for cross-references in the German archives and also by comparing Rauschnig's Hitler with the picture of the Führer which emerges from other sources. Though his pursuit of documentary check points turned up meager results, he was more successful in showing that Rauschnig's account fits tolerably well into the sweep of documentary Hitleriana extending from 1928 (*Hitler's Second Book*) to 1944–45 (*Hitler's Table Talk*). Aside from the general reservations that must arise whenever one document or source is compared with another, however, Schieder's vindication of Rauschnig is not beyond specific criticism. He fails to compare Rauschnig's version of Hitler's views with a recently published account that comes from approximately the same period (Edouard Calic, *Unmasked: Two Confidential Interviews with Hitler in 1931*). Furthermore, Schieder readily accepts at face value Rauschnig's explanations of his own motives and actions. Yet here and there where it is possible to check these assertions, the results do not always enhance one's confidence in Rauschnig. For example, Rauschnig repeatedly claimed, and Schieder accepts his assertions as valid, that in the postwar period he refused to aid the Allied war-crimes prosecutors because he held that the Nuremberg trial concept was faulty. In fact, however, during the summer of 1945 Rauschnig declared his readiness to testify and in a twelve-page summary specified the points he would be willing to make on behalf of the prosecution. Some may be ready to dismiss this as a trifling inconsistency, but if we are to extend much credence to Rauschnig's accuracy and his memory regarding Hitler's statements, it is difficult not to be troubled by errors or half-truths in the assertions he made about himself.

Schieder concludes that although Rauschnig's *Gespräche* does not constitute a verbatim record of Hitler's views, it may serve as a general summing up of those views in the early 1930s. This is a fairly guarded assessment, but in the opinion of this reviewer, a historian might do well to exercise even more caution than Schieder.

BRADLEY F. SMITH  
Cabrillo College

ERNST NOLTE. *Deutschland und der Kalte Krieg.* Munich: R. Piper & Co. Verlag. 1974. Pp. 755. DM 64.

Miracles do happen. Before reading Ernst Nolte's book, this reviewer would have considered it im-

possible still to write a Hegelian history in which events are determined by the logic of concepts. Such, however, is the underlying assumption of this book. According to Nolte, for example, the Bolshevik Revolution was neither regularly Marxist, because it did not happen in a highly industrialized country, nor entirely irregular because Russia was no longer an exclusively agricultural economy. For this reason, the Russian Revolution had to arouse a reaction that also was impure, a mixture of non-Marxist elements. "This answer, when it originated, received first accidentally the name Fascism, and if it had not originated . . . it could have been constructed in thought" (p. 130). Because of this Hegelian approach, the gist of Nolte's book, despite its awesome length of 755 pages, is a simple thesis: the Cold War began because at the end of the Second World War, Germany found itself split into two parts, and the Cold War ended when the German governments acknowledged this situation, that is, they accepted the existence of two German states—when the fact of partition had been raised into consciousness.

This spiritualized notion of history gives Nolte's book peculiar features. He places the events from 1945 to 1972 in a broad framework: the war of the Greeks against the Persians; the creation of an Eastern and a Western Empire through the coronation of Charlemagne; the origin of a Left and a Right in Europe in the eighteenth century, accentuated by the establishment of the United States. All these events, according to Nolte, fed into the Cold War. The chronological and spatial extension of the content, however, also serves the purpose of terminological clarification. The widening of the historical horizon permits Nolte to compare similar phenomena of different times, to distinguish among them, and, particularly, to separate the Cold War from other warlike struggles and tensions. Nolte is concerned with a clear definition of his concepts; he is aware that the concatenation of ideas, which he sees behind the factual course of events, requires terminological precision.

The book is concerned with ideas and the change of ideas during the Cold War. A reader expecting a detailed treatment of diplomatic negotiations and governmental actions will be disappointed. Nolte aims at describing the "atmosphere" in which events develop and not the events. Furthermore, when he discusses diplomatic documents, the accent is on discovering whether, in their formulation, they reflect a new "self-understanding," rather than on their practical or tactical purposes. Nolte's preferred sources are materials which document the development of public opinion: newspapers, periodicals, books, speeches, movements in political parties and at universities. The

value of Nolte's analyses is sharply diminished, however, by his method, or his lack of method, in the use of these sources. There is no systematic, certainly no quantitative, research about the treatment given to Cold War issues in leading organs of public opinion—an approach that might not only have demonstrated a gradual evolution in thinking about the Cold War, but also indicated the forces determining this thinking. He is selective and gives attention only to those statements that link the unfolding of the logical process to the final outcome. Walter Lippmann, George Kennan, Eugene Rostow, Paul Sweezy, Stanley Hoffmann, and Sidney Hook are some of the writers and publicists of this country whose statements Nolte uses to indicate the various stages in thinking about the Cold War. Because quotations are taken very much out of context, the reader gets no clear notion whether the quoted statements are representative of politically important groups or express merely a personal conviction. For instance, statements from Kennan's writings are cited to document a variety of divergent positions and are not connected with the events which influenced the evolution of his thought.

Nolte's method of basing his story on expressions of public opinion meets particular difficulties when he begins to analyze Russian policy. The few published manifestations of Russian political thinking are overinterpreted, and Nolte's procedure reminds one of the excesses of our own "Kremlinology." As can be expected, his method is most successful in outlining the development of political thinking in Western Germany during the Cold War period; here too, the lack of systematic analysis and an impressionistic approach placing too much emphasis on the author's own experiences at the Universities of Marburg and Berlin make the results of Nolte's discussion tentative. But Nolte suggests connections which students of post-war Germany will find useful to follow up. German affairs also produce Nolte's one excursion into institutional history—an informative description of the bureaucratic apparatus created in Western and Eastern Germany for the conduct of the Cold War.

It is not without purpose that Nolte has placed the events of the Cold War in a broad framework, including the ancient world, and stressed only those facts which were steps toward the war's logical conclusion. This is contemporary history, written *sub specie aeternitatis*; Nolte tries to isolate and emphasize those facts on which, he assumes, historians of later centuries will focus for a comprehension of the history of our time. But the domination of the entire story by its outcome creates the impression that political leaders were confronted by a chess problem which, at the end,

they were able to solve. Nolte eliminates the moral element from political actions and describes them as if they were pure technics. Events are not bound up with personalities, with economic interests, with different modes of thinking or political traditions and values; everything is interchangeable if external circumstances are the same. If the Communist party in the United States had been as large as that of Germany, Roosevelt would have sympathized with anti-Semitism (p. 160); the only difference between Auschwitz and the American conduct in Vietnam was that the Americans could protest, the Germans could not (p. 528); anti-Semitism can be regarded as a by-product of capitalism and can develop anywhere. Nolte does not justify crimes and vices, but in considering all happenings in the political area from a functional point of view, he denies the impact of moral considerations, emotions, and values in political life. It may be praiseworthy to be objective, to "extinguish one's self," but when, as in Nolte's case, the fanaticism for objectivity goes so far as to look upon things from a height which makes differences between icebergs and volcanoes no longer discernible, the picture is wrong. One may take a revisionist attitude on the Cold War, as this reviewer does. Nevertheless, it cannot be denied that moral convictions, fears, and economic interests were involved in the crises which punctuated the years of the Cold War and played their role in the deadlocked end. A history of the Cold War which does not consider and evaluate the importance of these factors is not true to life.

There are reasons for this distorted perspective. Nolte's book is more than another study of contemporary events; if it is not the first, it is at least one of the first German historical books after the Second World War which returns to the traditional, nationalistic pattern of pre-World War German historical writing. The book deals with "Germany and the Cold War," not only because from the German angle some new light might be thrown on the events, but also because Nolte considers Germany the center of the Cold War and the German acceptance of their country's partition as the decisive event concluding the Cold War. The message of the book is that Germany has become again an important center of the political world. But the decisiveness of Germany's role in the Cold War, which, according to Nolte, previous writers have not recognized, but which to him is evident and clearly a determining factor in his writing this book, actually is a most doubtful assumption. At least, two questions must be raised. The one is whether, in the events of the Cold War, and particularly in the struggle for Berlin, Germany and Berlin were really the objects of the struggle or only symbols of a much wider

global conflict. No serious discussion of this question will be found in Nolte's book. Moreover, as the strange mixture of names which he considers representative of American public opinion shows, Nolte has only foggy ideas about the manner in which policy is made in the United States. He is remarkably vague and unsatisfactory about the global interests and involvements of the United States; without serious examination, he subordinates them to the struggle for Germany.

But there is another question closely connected with the previous one. When did the notion of the need to accept a partition of Germany take hold in the West? Nolte suggests that the recognition of the partition by the two German governments was the crucial event, from which the acceptance of the partition by the other powers followed. But voices—influential voices—willing to accept a divided Germany, were heard among the Western powers since 1946, and one could probably make a good case that, in accepting the division, the two German states, and particularly Western Germany, did not initiate a new development, but gave up a position lost long before.

It is no accident that these questions do not receive the treatment they deserve; the history which would have taken such considerations into account would not have been a logical process. It would have been a very different history into which interests and personalities, emotions and miscalculations, passions and moral considerations would have entered, and in which the varieties of possibilities which existed in these years would have been appropriately weighed. This would have been a book in which the "atmosphere" of these post-war decades would have come much more to life than in the obfuscating dispassionateness of Nolte's book.

FELIX GILBERT

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FRANCESCO CARACCILO. *Uffici, difesa e corpi rappresentativi nel Mezzogiorno in età spagnola*. Reggio Calabria: Editori Meridionali Riuniti. 1974. Pp. 264. L. 6,500.

For two hundred years Spain ruled the Kingdom of Naples as a viceroyalty. Gradually, during this period the ancient rights of the Neapolitan people, guaranteed by Ferrante of Aragón in 1483 and confirmed in two edicts by Charles V, were whittled away. Offices that were to be granted only in consultation with the Neapolitan *Consiglio Collaterale* and other local advisory and judicial bodies were disposed of in Madrid. Taxes eroded the wealth of the kingdom and went to fill the seemingly unending demands of Spanish imperial ambitions. What in the 1500s had been the attempt

of an increasingly absolutist government to create an efficient administration by closely controlling local officials became, in the following century, a venal, corrupt, and cumbersome state bureaucracy.

Working almost exclusively with the rich depositary of sources in the Simancas archives, as well as with the collections of Naples, Reggio Calabria, Cosenza, and various other archives and libraries, Francesco Caracciolo has given us a careful study of how Spain ruled Naples and how increasingly the interests, guarantees, and powers of the representatives of the local population were subordinated to purely Spanish policies.

Caracciolo points out that the attempt at monarchical centralization of power and the seventeenth-century crisis were not limited to Naples, but are to be considered in the light of developments throughout Western Europe in the same period. Thus, this study—a careful analysis of governmental practices or malpractices in a small state—adds an important piece to the European mosaic composed of national histories. For students both of Italy and Spain, this work belongs on their library shelf with Koenigsberger's *Government of Sicily under Philip II of Spain: A Study in the Practice of Empire* and the many other works by Italian and foreign scholars on various aspects of life, government, and politics in Naples during the centuries of Spanish rule.

EMILIANA P. NOETHER  
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CARLO M. CIPOLLA. *Cristofano and the Plague: A Study in the History of Public Health in the Age of Galileo*. Berkeley and Los Angeles: University of California Press, 1973. Pp. 188. \$7.50.

Epidemic outbreaks of contagious disease—whether Biblical leprosy and medieval plague or smallpox, yellow fever, cholera, and influenza in more recent times—have repeatedly challenged the leaders of Western communities to deal with a fearful threat. Contagion was an obvious phenomenon that had to be countered, even though its mechanism was not understood until the mid-nineteenth century.

The little town of Prato in the Grand Duchy of Tuscany sprang into action at the first news of plague, on October 26, 1629. With only 6,000 inhabitants—17,000 for “greater Prato”—it boasted four physicians and three surgeons. In the emergency, the government appointed four (later eight) lay “health officers,” headed by Cristofano Ceffini. His *Libro della Sanità*, composed shortly after the epidemic subsided, has provided Carlo Cipolla with unique source material.

Prato, on orders from Florence, took the usual

preventive measures. It stationed guards at all mountain passes, fords, and city-gates, isolated the houses where patients had died, and quarantined their families for twenty-two days. A pest-house and a convalescent home, requisitioned after intensive haggling, were staffed and run at municipal expense. Cristofano supervised the feeding of the survivors, who lived in boarded-up houses, the burial of the dead, the fumigation of their homes, and the burning of their belongings. Cristofano reports that twenty-five percent of Prato's inhabitants died of the plague in 1630–31.

In Carlo Cipolla, Cristofano found an appreciative chronicler, even a friend. Economist, historian, demographer, professor at the Universities of Pisa and California at Berkeley, and increasingly intrigued by the impact of epidemic disease on European communities, Cipolla relishes Cristofano's exact figures, his parsimony, his public spiritedness, his circumspection and common sense. While Defoe or Camus may tell us more about the psychological impact of plague, and Bowsky gives us a wider purview in *The Black Death*, this fine short book, written with verve and sharp critical acumen, with its eight factual appendices and a full—mostly Italian—bibliography, should serve to tempt more investigators into the field of public health and medicine where social historians can reap rich harvests.

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GAETANO FALZONE. *La Sicilia nella politica mediterranea delle grandi potenze*. Palermo: S. F. Flaccovio, Editore, 1974. Pp. 428. L. 8,000.

This thick octavo paperback tells how the Sicilian 1848–49 bid for separation from mainland rule fell disastrously afoul of Anglo-French rivalries in the Mediterranean. Falzone teaches at the University of Palermo, and his study, twenty-five years in the making, complements nicely two volumes edited by Federico Curato on the Sicilian revolutionaries' correspondence with London and Paris. While Curato shows the Sicilians through their own diplomatic papers, Falzone elaborates the French connection as preserved in the Quay d'Orsay files. A proper balance of 250 pages of commentary in Italian and 150 of documents in French offers both narrative and source material elucidating the episode. The author looks at public opinion in Sicily on the eve of 1848, the uprising of January 12, the mission of Lord Minto, the policy of the Second French Republic, the collapse of Neapolitan rule, the election of a Piedmontese candidate to the throne (he refused), the Bourbon Ferdinand's bombardment of Messina, the French arrange-



ments for a cease fire, and the fall of the provisional government. His conclusion is blunt: "Sicily was by fate just one element of the Mediterranean game, hence the bitter revolutionary lesson warned that only through rejoining the peninsula could the island hope to preserve itself from the very rude injuries of international rivalry." Falzone is recommended.

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SIDNEY SONNINO. *Diario*. Volume 1, 1866-1912, edited by BENJAMIN F. BROWN, introduction by GIORGIO SPINI; volume 2, 1914-1916, edited by PIETRO PASTORELLI; volume 3, 1916-1922, edited by PIETRO PASTORELLI. Bari: Editori Laterza; Lawrence: University Press of Kansas. 1972. Pp. xlv, 534; xii, 374; xi, 407. \$35.00 the set.

SIDNEY SONNINO. *Scritti e discorsi extraparlamentari*. Volume 1, 1870-1902; volume 2, 1903-1920. Edited by BENJAMIN F. BROWN. Bari: Editore Laterza; Lawrence: University Press of Kansas. 1972. Pp. xxii, 932; xv, 936-1716. \$45.00 the set. *Diario* and *Scritti*, \$75.00 the set.

Sidney Sonnino had charge of the Italian foreign office from November 1914 to June 1920. So long a tenure, preceded by almost half a century of active participation in Italian politics—he was twice briefly prime minister—makes him a personage of unusual importance to whom surprisingly little attention has been given. This has been partly due to the fact that, except for the collection of his parliamentary speeches, few of Sonnino's writings could be found. That the present collection should be the product of American scholarship, points up the increasing role played by Americans in European historiography. The finding of Sonnino's papers in a hidden backroom of his villa at Montespertoli near Florence makes a fascinating tale of sleuthing, which is attractively related in Spini's introduction. The five volumes under review consist of two distinct parts: three of them the diary, covering the period 1866-1922; the other two some seventeen hundred pages of his writings and speeches other than those delivered in parliament. Through the latter, one can relive virtually the entire period of prefascist united Italy. Sonnino's interests were broad, and he was a cultured man. His two essays on the *Divine Comedy*, especially the one on Beatrice, reveal a facet of sensitivity not usually associated with him.

But Sonnino was above all a political animal. Although often regarded as a rigid conservative, his direct experience with the Paris Commune of 1871 made a lasting and conditioning impression on him. His concern with the "social question" remained ever alive; very early he advocated extending the franchise, even to illiterates, a very

radical position at the time. The problem of the South and the issue of land tenure in general loomed large among his preoccupations.

On the score of state finance, however, he definitely believed in the orthodoxy of balanced budgets. Wedded to all aspects of free enterprise, he saw in socialism a great potential menace. Interestingly, he would also have subscribed to the Gambettan slogan, *le cléricisme voilà l'ennemi*, and he felt that the existing ambiguity of relations with the Vatican, embodied in the Law of Guarantees, was the best possible arrangement, far preferable to a formal agreement.

As Minister of Foreign Affairs, Sonnino's views on foreign policy are of greatest interest. His earlier position on the score of Italy's place among the powers forecast his subsequent activity. Like most other directors of Italian foreign policy, Sonnino's appreciation was a moderate one based on a realistic appraisal of Italian capabilities. He had believed in the Austro-German alliance but, once at the foreign office, he became convinced that the Allied camp offered the better prospects.

Sonnino was cautious, believing in the letter of the law. The American intervention in World War I and the Wilsonian program for peace raised some obvious problems for Italy. Even before that intervention he saw Wilson's activity as designed "already to secure a place at the peace congress" (*Diario*, 3: 103). One can only regret the absence of comments on the Fourteen Points speech and of any mention of the discussions between the Allies and Colonel House in connection with the German armistice. It was on that occasion that the ambiguity arose concerning Point IX—"a readjustment of the frontiers of Italy should be effected along clearly recognizable lines of nationality"—that was to bedevil the subsequent peacemaking and lead to the Italo-American clash in April 1919. Nor is there any mention of Wilson's visit to Italy in December 1918, and surprisingly little on the peace conference. Sonnino would have taken his stand on the 1915 Treaty of London, a more solid position than Orlando's bungled attempt at compromise, though little more likely to have proved successful. Sonnino himself signed the Treaty of Versailles, but by that time the Orlando ministry had already fallen, an event that virtually ended Sonnino's political activity.

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C. J. LOWE and F. MARZARI. *Italian Foreign Policy, 1870-1940*. (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1975. Pp. xi, 476.

This is a contribution to a series which already includes volumes on British, French, and Austro-



Hungarian diplomacy. Each book offers a synthesis of the latest research supplemented by the results of the authors' own forays into the archives, and also contains an English-language appendix of the most important documents.

Certain shortcomings inherent in the series format, which have been noted by reviewers of earlier volumes, are present here. First, it is not immediately apparent for whom *Italian Foreign Policy* is intended. Too detailed to attract the layman or undergraduate, the book does not, however, tell the specialist much that he does not already know. To be fair, the graduate student, aspirant to become a specialist, will find it an invaluable reference work; hence, an audience does exist, albeit a limited one. Second, the rationale behind the choice of some topics to receive documentation from primary sources and others not remains unclear. Sometimes a monograph is cited; sometimes documentary material which can be found in the footnotes of the same monograph. Fundamentally this work, and series, are synoptic; it might be better to admit it more openly.

These caveats aside, this volume merits high praise. Starting from the conventional description of Italy as the "sixth wheel" of European diplomacy, Lowe and Marzari trace the interplay of two Italian schools of thought. One, cognizant of Italy's economic and strategic deficiencies, strongest in the north and preoccupied with the German problem whether Hapsburg or Nazi, urged diplomatic caution. The other, hypernationalist and radical, dreamed of glory and empire to the south in Africa regardless of Italian capacities. The former, in the persons of Visconti Venosta, Robilant, San Giuliano, and the permanent staff of the foreign ministry, tended to hold sway before 1914, as they sought and secured maximum "flexibility" within the Triple Alliance (p. 71). The latter, heralded by Crispi, found its true champion in Mussolini who "substituted a strategy for a policy . . . until the means, the acquisition of strength, became an end in itself" (p. 209).

The skill with which these two themes are handled reveals the loss to the historical profession occasioned by the premature death of the authors—both Canadian scholars in mid-career who have died recently just as their joint work was reaching fruition. This sad coincidence must be held to account for some rough edges on an otherwise commendable book: an abrupt conclusion, misspellings in the notes, some missing German titles in the bibliography. Secondary characters are not always identified in the text; and the index, surely the publisher's responsibility, omits many of the *dramatis personae*.

ALAN CASSELS  
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DORA MARUCCO. *Arturo Labriola e il sindacalismo rivoluzionario in Italia*. (Studi, number 10.) Turin: Fondazione Luigi Einaudi. 1970. Pp. 350. L. 4,000.

This is the first full-length biography of an interesting, amazingly contradictory, and ultimately not very important man. Consider the following information: born in Naples in 1873; already a member of the Neapolitan anarcho-socialistic left in the mid-1890s; forced into exile after the *fatti* of 1898; student of Pareto in Switzerland; a major leader of the revolutionary syndicalist movement to 1908; a passionate proponent of the Libyan War of 1911; and, of course, an interventionist in the Great War.

And this is only the beginning! This veteran deputy of four legislatures—as an "independent" socialist, 1913–21—became for a time a champion of the Russian Revolution, defined as an "interesting experiment in a government exclusively of the working classes" (p. 249). Then, to the astonishment of all, in 1920 he became the Minister of Labor in the last Giolitti government.

A characteristic example of Arturo Labriola's tergiversations was his attitude toward fascism and the fascist regime. In early 1922, straining to emphasize what was "positive" in fascism, he insisted that "fascism does not fight socialism, but the socialist party; not democracy, but its institutions and the traditional democratic party. It would itself like to be a new democracy and a new socialism" (p. 280). The presence within fascism of his old syndicalist comrades (Orano, Lanzillo, Rossoni, Panunzio) undoubtedly influenced this judgment. On the other hand, Dora Marucco's research has revealed that Labriola made one of the most eloquent and timely protests against fascism. On November 12, 1921, he wrote to V. E. Orlando: "I've been in Rome during these last tumultuous days and I left with a sensation of horror which still oppresses me. . . . To sum it up: why pay taxes, have recourse to justice, participate in elections and have a government when we permit the construction of an armed power, fascism, in the country. . . . If you do not decide to disarm fascism, then fascism will destroy the State: either directly by ruining the State, or indirectly by provoking an armed resistance of the socialists and communists which has now become even a question of the right to life" (pp. 328–29). Several times during the next couple of years, Labriola stressed to Giolitti and others that fascism as a "private army" could be defeated only by armed force. That this was not done is a measure of the bankruptcy of the liberal state of that time.

And yet by 1926 he is negotiating with the fascists to exchange his promise of a withdrawal from politics for either a legitimate passport or permission to accept an offer to teach in Italy. The

government declined the bargain, and Labriola escaped to France. There and in Belgium he quarreled incessantly with antifascists of all colors, openly defended the Regime with regard to the Abyssinian War, and returned to Italy in 1936.

After the Second World War, he became a member of the Constituent Assembly and a Senator by right because of his four terms as a deputy in the prefascist parliament. He was one of the very few Italian politicians not belonging to the "extreme left" who vigorously attacked the Atlantic Pact. On this ground, in 1956, he ended his political career as an "independent" in the Communist list for the municipal council of Naples!

There are many fine things in this biography of that paradoxical figure. Marucco's work is strongest in its exposition of Labriola's career to 1908 as a revolutionary syndicalist, especially in her demonstration of that movement's weakness as an independent force in Italian labor. Unfortunately, however, she does not explain satisfactorily (perhaps no one could!) the chameleon-like character of his life. Ultimately, it is hard to improve on Filippo Turati's judgment of 1925 that Arturo Labriola "will always be a headache to whatever party he belongs."

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ORNELLA CONFESSORE. *Conservatorismo politico e riformismo religioso: La "Rassegna Nazionale" dal 1898 al 1908*. (Saggi, number 101.) Bologna: Il Mulino. 1971. Pp. xii, 466. L. 6,000.

ANGELO GAMBASIN. *Parroci e contadini nel Veneto alla fine dell'Ottocento*. (Biblioteca di storia sociale, 1.) Rome: Edizioni di Storia e Letteratura. 1973. Pp. 316. L. 9,000.

No one has ever doubted the importance of the Catholic Church in modern Italian society; yet until a few years ago, only a handful of books had explored the subject seriously. Recently, scores of useful studies have treated particular groups of Catholics, the Church and politics, and religious life in various regions. Each of these books represents an important aspect of that historiographical vitality. Confessore studies the ideas of an influential group and with finesse assesses their political and religious attitudes (appropriately, Fausto Fonzi wrote the introduction). Gambasin uses records of episcopal surveys to build a pioneering analysis of the relation between religious behavior and social structure in the Veneto (appropriately, Gabriele de Rosa wrote the introduction to this volume, the first in a new series).

Both books are based on impressive original re-

search; both have footnotes nearly as extensive as the text, connecting each point with other studies of every sort; both are methodologically self-conscious, and both deal with the critical period around the turn of the century when Italy's liberal regime was rocked by growing social and political conflict.

Confessore studies the *Rassegna Nazionale* in its most vital period, 1898-1908 (it was published from 1879 to 1943 and again from 1950 to 1953), describing both its public positions and the voluminous correspondence of its small group of supporters. Catholic patriots who dreamed of reconciling a conservative Risorgimento with a Church attuned to modern ideas, theirs is a history of loyalties baffled and frustrated. They were tainted with "Americanism" when that was condemned by Leo XIII, tarnished and frightened by Pius X's campaign against modernism ten years later. Consistently drawn to a strong state that would combat socialism, strikes, and the vaguer threat of democracy, they could comprehend neither the attacks on Catholics launched by Di Rudini and Pelloux nor the rising Christian Democratic movements. By 1908 they attempted to launch a Conservative Reform party, just as Giolitti was widening the scope of Italian politics. Despite reassuring connections with the royal family, the men of the *Rassegna Nazionale* were increasingly isolated. Their social connections made them important to contemporaries (their subscribers never numbered more than 600), but it is their religious and political isolation that is historically significant. That, for all her balanced and careful exposition, Ornella Confessore does not adequately explain.

Gambasin on the other hand, who has written about Catholics in the Veneto before, writes history from below (de Rosa's introduction even includes a bow to the *Annales* school). He constructs maps of the percentages of the population making regular confession (a range from 40 to 96 percent) and from these identifies three zones in addition to urban areas, and then describes each in terms of geography, land-holding, and religious practice. He depicts a lower clergy active, anti-liberal, and opposed to the aristocracy, battling local superstitions and dangerous ideas spread by returning migrants and by freemasons and socialists in dance halls and bars. A mine of insights, this painstakingly assembled book shows much about both the Church's vitality and the universal conviction that it was mortally threatened. Fundamentally, however, the connections made between religious practice and social conditions are those the priests perceived, and the emphasis is therefore on the Church's response rather than Catholicism as a social agent. Social reality and the views of the clergy become so entangled that Gambasin's in-

telligent and quite credible descriptions, region by region, remain unconvincing and impressionistic. An appendix rich in statistics and documents even offers a basis for alternative explanations.

Thus both books provide new information about and important insights into the complicated responses of Italian Catholics (from the preoccupation with migration to the influence of Rosminian ideas) to a society experiencing disruptive change. But neither work is clear as to how its findings should be applied. Although sophisticated and informed, they fall short of the kinds of argument that could lead to the generalizations that historians still lack.

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OTTAVIO BARIÉ. *Luigi Albertini*. (La vita sociale della nuova Italia, volume 21.) Turin: Unione Tipografico-Editrice Torinese. 1972. Pp. x, 569. L. 7,000.

The Albertini family allowed Ottavio Barié sole access to the Albertini Archives in Milan to facilitate the writing of this biography. Barié subsequently edited a four-volume set of Albertini's correspondence. The work currently under review helps place Albertini on the center stage of Italian history where he belongs. English-speaking scholars recognize Luigi Albertini as the author of the highly acclaimed *Origins of the War of 1914*, but not enough is known of his tremendously influential role as co-owner and director of Milan's *Corriere della Sera*, nor of his stature as one of Italy's leading liberals. This book is valuable not only because of the light it sheds upon Albertini, but also because it adds to the continuing debate on the Giolittian era and the origins of fascism, although the author misses the opportunity to place Albertini and fascism in a broader European context.

It is Albertini's opposition to the democratic liberalism of Giovanni Giolitti and his position in the crisis of liberalism and the rise of fascism that chiefly merit Barié's attention. He overemphasizes Albertini's "abstract moralism" as the basis of the ever-growing hostility to Giolitti, concluding that it was a question of methods more than the threatened consequences. This leads to the related contention that the *Corriere* was "an organ of information" rather than a political organ. In fact from 1898 to the triumph of fascism in 1922, Albertini and his prestigious daily called not only for law and order and the preservation of the Liberal State of the Risorgimento, but also for the continued exclusion of "subversive forces" outside the *Statuto*, including both socialist and Catholic masses. The *Corriere* consistently reflected the conservative lib-

eral tendency of the Italian ruling class. Giolittian policies of reconciliation with the laboring classes represented for Albertini the death of liberal Italy.

In the author's chapter on the fascist period, he has marshalled the historical facts and attempts to deal objectively with a complex phenomenon—an ambitious undertaking which is exhaustive, but not definitive. The analysis is flawed not so much because it is more or less an apology for his protagonist's initial support of fascist reaction (after all, Albertini redeemed himself in the eyes of his most vociferous critics for his courageous antifascist opposition from 1923 to 1925), but because it fails to deal with Albertinian conservative liberalism along lines indicated in the evidence contained in preceding pages. Albertini tragically witnessed the fruits of his rigid adherence to classical nineteenth-century liberalism. Always antiapocalyptic in nature, his liberalism opposed democratic liberalism, and opted for authority and order to resolve Italy's woes. This was evident during the Libyan conflict and the nationalistic effusion of the period. It was heightened during the Great War. And when Mussolini offered what seemed to be a viable alternative to Giolittianism, Albertini, who underwent another turn to the right after the peace, welcomed it as an antidote.

Barié spends nearly one hundred pages explaining the reasons for Albertini's "genuine myopia"—his familiarity with only the Milanese "official" fascism of Mussolini and *Il Popolo*, rather than the violent agrarian fascism of Emilia; Albertini's preoccupation with the Adriatic Question, and his absence as a delegate to the Washington Naval Conference; the failure of the ruling class to oppose fascism at first, although Albertini's position was more guarded and limited than, for example, Salandra's; and the excuse that no one could have known what a disastrous path fascism would take in the future. Albertini, Barié concludes, opposed belligerent nationalism, but he saw fascism as the extreme of a national party emerging during the interventionist struggle of 1914–15. He also had to direct himself to his middle-class constituency (referring, of course, to the "santa reazione" of middle-class public opinion to the fascists). Finally, Albertini thought fascism could be normalized and constitutionalized and save Italy from the Red menace.

There is lacking in this interpretation a sense of the organic crisis of Italian liberalism in this era, the incapacity of the governing class and its institutions to deal effectively with either democracy or fascism. With reference to Albertini specifically, the nationalist rhetoric he published in the *Corriere* (especially that of his dear friend, Gabriele D'Annunzio, a relationship treated superficially by Barié) during the Libyan conflict and the Great

War helped create a climate of brute force and cannot be cancelled out by his later renunciation of irredentism. His postponed attacks against Mussolini cannot erase his responsibility in preaching for strong countermeasures to rule by Giolitti or collaboration with socialists. He always took what he considered the moral position, based on faith in political formulas that had succeeded in the past for the historical Right, and he was forced to deal with the results in the turbulent first quarter of the twentieth century when those formulas simply did not work.

It is hoped that Barié's vast study will lead to further research on Albertini and to a better understanding of fascism and of the Giolittian era, a rich and complex period in Italian history, which was once, like Luigi Albertini, a neglected child of historical scholarship.

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MELTON S. DAVIS. *Who Defends Rome? The Forty-five Days, July 25-September 8, 1943*. New York: Dial Press. 1972. Pp. xii, 560. \$12.50.

This is an overlong, popularized account of the period from the coup d'état that overthrew Benito Mussolini's Fascist dictatorship on July 25, 1943, to the announcement of the Italian armistice with the Allies on September 8, the eve of the invasion at Salerno. During those forty-five days two aged Machiavellians, Victor Emmanuel III and Marshal Pietro Badoglio, headed a royal dictatorship which sought ineptly to negotiate a *volte-face* with the Anglo-Americans while trying to reassure Hitler that Italy remained loyal to the Axis. A few hours after Badoglio broadcast news of the armistice, he and the panic-stricken royal family fled from Rome in a motor caravan to the Adriatic coast, leaving behind no coherent orders for defending the capital from a German take-over.

The day-by-day events of this period are recounted in cinematographic fashion by the author, an American journalist who has spent many years in Rome as a correspondent for the Mutual Broadcasting System. Davis has also written *All Rome Trembled* (New York, 1957), a journalistic account of the Wilma Montesi scandal that rocked Italy's Christian Democratic government in 1954. In his most recent book, Davis spares no criticism of the stupidity, duplicity, and even cowardice of many of Italy's leaders during the summer of 1943. His technique is to set forth in successive chapters accounts of what was going on concurrently in Italian government and opposition circles as well as in Allied and German headquarters. With an eye for colorful conversation and interesting though often inconsequential details, Davis has

pieced together his book from an impressive array of printed sources and, in certain cases, personal interviews. Numerous photographs are included.

Apart from its excessive length, the book's chief shortcoming is a failure to place the "forty-five days" in the larger context of Italian history. One finishes the book without having gained a clear picture of why Italy found herself in such a mess in mid-1943 or what the political and military repercussions of this interlude were going to be. Although Davis's account may interest the general reader who has some acquaintance with Italy, it does not replace the more scholarly studies of F. W. Deakin, *The Brutal Friendship* (New York, rev. ed., 1966); Col. Albert N. Garland and Howard McGaw Smyth, *Sicily and the Surrender of Italy* (Washington, 1965); and Luigi Ganapini and Massimo Legnani, *L'Italia dei quarantacinque giorni* (Milan, 1969).

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I. B. GREKOV. *Vostochnaia evropa i upadok zolotoi ordy (na rubezhe XIV-XV vv.)* [Eastern Europe and the Downfall of the Golden Horde (at the Turn of the Fourteenth and Fifteenth Centuries)]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 519. 1 r. 93 k.

I. B. Grekov, the son of Academician B. D. Grekov, is a senior fellow (*starshii nauchnyi sotrudnik*) of the *Institut slavianovedeniia*. His ambitious monograph can be divided into two parts. The first (pp. 13-310, with the brief Conclusion, pp. 483-87) is a detailed and analytic political history of Eastern Europe—including Poland and the Golden Horde—emphasizing the parallel evolution of the Grand Principalities of Vladimir and Lithuania in the late fourteenth and early fifteenth centuries. The second part (pp. 311-482) is a wide-ranging discussion of Muscovite ideology during the same period. Both sections (occasionally verbatim) derive from and expand upon Grekov's earlier works, the former on his *Ocherki po istorii mezhdunarodnykh otnoshenii Vostochnoi Evropy XIV-XVI vv.* (1963), pp. 13-118, and the latter from his articles in *Sovetskoe slavianovedenie* (1970, number 6) and in *Pol'sha i Rus'* (1974).

Grekov emphasizes the impact of the complex interaction of all East European states and principalities. He shows how the changing configuration of East European politics influenced the centrifugal and centripetal forces and tendencies toward multi-ethnic or mono-ethnic polities which were everywhere at work. Grekov's forte is reinterpretation of seemingly minor events so that they reflect



and validate much larger processes and international relations; the events leading up to and following the battle of Kulikovo (1380) are an excellent case in point. He sometimes minimizes internal causation, however, and over-rationalizes, as, for example, his assertion that every "Tatar" raid was a result of conscious Horde policy based on the balance of power in Eastern Europe. Moreover, Grekov sometimes slights the full picture of major events, such as the Union of Kiew, or processes, such as the weakening of the Golden Horde alluded to in his title. There are obviously no surprises in the outcome—the ascension of Moscow, the decline of Lithuania and the Golden Horde—but since Grekov had earlier dated the fragmentation of the Golden Horde to the mid-fifteenth century, 1420 is not a propitious point to end this narrative. In addition, specialists will quite properly raise innumerable questions of detail, since Grekov's willingness to force the evidence and to rely on highly indirect substantiation, is well-known.

The inclusion in the book of a discussion of Muscovite ideology is justified by Grekov's demonstration of the intimate connection between ideological texts and political history. His premise that differing presentations of the same event, e.g. the battle of Kulikovo, in different sources embody alternative ideological programs or phases reacting to a rapidly changing political reality, are fully warranted. His datings and interpretations, however, of various monuments will arouse controversy. Besides, Grekov does not discuss several issues that require more attention, such as the Russian appreciation of the Chingissid blood legitimacy. But Grekov's close textual reading and fertile imagination often permit him to draw hitherto unnoticed convincing and illuminating connections.

The monograph is based upon comprehensive use of published sources and an extensive bibliography, although there are a few curious and inexplicable omissions. Most importantly, Grekov writes with flair. The book makes stimulating reading.

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E. PAMLÉNYI, editor. *Social-Economic Researches on the History of East-Central Europe*. (Studia Historica Academiae Scientiarum Hungaricae, number 62.) Budapest: Akadémiai Kiadó. 1970. Pp. 231. \$7.20.

This collection of four essays deserves recognition, however belated, because of the remarkable contribution in English by Laszlo Katus. While among the other three essays there are two coauthored by

Hungary's justly renowned economic historians, Iván Berend and György Ránki, the findings of their useful essay in German on Hungarian national income and capital accumulation, 1867–1914, are available, along with much more, in their *Hungary, A Century of Economic Development* (1974). Their essay in Russian on the Industrial Revolution in Eastern Europe likewise appears in a more expanded form in their *Economic Development in East-Central Europe in the 19th and 20th Centuries* (1974).

The great strength of Katus' essay is its scrupulous use of statistics, with 36 pages of invaluable tables and graphs appended, to measure the extent and nature of "modern economic growth," precisely as Simon Kuznets defined it, in dualist Hungary between 1867 and 1914. Finding an annual growth rate over this long period of 2.2 percent in real per capita terms for a near equivalent of GNP, Katus has established that the Hungarian economy achieved a Rostowian "take-off" before World War I, comparable in Eastern Europe only to the Russian and Czech experiences. Gains in productivity accounted for fully half of this average increase, principally through more intensive agriculture, in contrast to Russia, and through the rise of railway-related heavy industry. These impressive gains in productivity lead beyond physical increases in labor and capital to the improved quality of labor and the size of the market as well as the introduction of better machinery, as Katus demonstrates. He emphasizes the pre-1914 improvement in literacy and technical training and goes so far as to stress the economic advantages of Hungarian inclusion in the Habsburg Dual Monarchy, until recently an unthinkable position among Hungarian scholars. Among these advantages, the author argues convincingly, were free movement of factors of production, including skilled labor, and the free market of 50 million people furnished by the monarchy's customs union.

A concluding section compares pre-1914 Hungarian growth to that of the rest of Europe and places the author's work in Marxist focus. Katus suggests that continuing Hungarian backwardness in 1914, relative to the developed economies, should not be blamed on the nineteenth-century experience within the Habsburg monarchy, but rather on the Hungarian role of agricultural supplier to Western and Central Europe. This role evolved during the early modern period, according to post-1945 Hungarian historiography and to the recent and much less original than advertized works of Immanuel Wallerstein.

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JOSEPH ROTHSCHILD. *East Central Europe between the Two World Wars*. (A History of East Central Europe, volume 9.) Seattle: University of Washington Press. 1975. Pp. xvii, 420. Cloth \$14.95, paper \$7.95.

This is the ninth volume in a projected eleven-volume multi-author series entitled "A History of East Central Europe."

The term "East Central Europe" is apt to bring an uncomprehending frown to the faces of even many professional historians, for it is quite new. It has come into use only since World War II, and has not gained general acceptance yet. The late Oscar Halecki first promoted it (see, for example, his book *The Limits and Divisions of European History* [New York, 1950]) to designate the area of Europe, which the editors of this series define as having "the eastern linguistic frontier of German- and Italian-speaking peoples on the west, and the political borders of Rus/Russia/the USSR on the east." To designate the same area, some historians still use the term "Central Europe," others the term "Eastern Europe," while journalists, economists, and political scientists often prefer the terminology of the cold war and call it the "Soviet Bloc." Central Europe implies, however, the inclusion of Germany, and Eastern Europe the inclusion of Russia, while the Soviet Bloc implies an area homogeneous in character and internally solid. In reality, East Central Europe has never had any unity until that which was imposed on it by the Soviets after World War II. The solidity of the Soviet Bloc was more apparent than real even in the heyday of Stalinism, and since the death of the Soviet dictator in 1953, all of the countries of East Central Europe have to a varying degree reasserted their individuality.

Historically, East Central Europe has witnessed a great mingling of Slavic, Germanic, Latin, and other peoples. Roman Catholicism and Greek Orthodoxy, but also Protestantism and Islam, have won adherents in the area. Parts of the area for centuries were under the rule of the Ottoman empire, the Habsburg empire, the Russian empire, the Polish-Lithuanian commonwealth, Prussia, and the mercantile republic of Venice, all of which have left a permanent impress on it. The peoples of the area have reached very different levels of social, cultural, and economic development, from the high level of the industrialized and urbanized Czechs to the modest level of the pastoral and tribal Albanians.

This great diversity of East Central Europe creates particularly difficult organizational problems for the historian trying to write the history of the entire area. Many historians will question the organization of the series as a whole, as well as of its individual volumes. Despite the wish of the editors to avoid the national approach to the his-

tory of East Central Europe, Rothschild finds it impossible to present an integrated history of the area between the two World Wars and treats it country by country. This reviewer says this, not in criticism but in recognition of the difficulty of the task, as he has taught and written on the history of East Central Europe, and has also found it impossible to abandon the national approach. Rothschild covers in his work Poland, Czechoslovakia, Hungary, Yugoslavia, Romania, Bulgaria, Albania, and the Baltic states. His exclusion of Greece on the ground that it is culturally, economically, and politically more a Mediterranean than a Balkan country is more convincing than his exclusion of Austria on the ground that it is not culturally, economically, and politically part of East Central Europe and that it was absorbed in the problems of *Anschluss* between the World Wars. This is a case of projecting Austria's outlook since World War II when it resolutely turned its face westward back to the interwar period. At that time, however, Austria still had more cultural and economic ties to other successor states, notably Hungary and Czechoslovakia, than, let us say, Prussia, and politically was not completely absorbed in the problems of *Anschluss*, but oscillated in Hamlet-like indecision between *Anschluss* and Habsburg restoration. This reviewer recognizes, however, that a certain amount of arbitrariness is inevitable in delimitating area history.

Rothschild has avoided the pitfall of trying to write a perfectly symmetrical history of the countries of East Central Europe. Instead, he presents a well-balanced treatment, emphasizing those aspects of each country which were most important. Sometimes he sidesteps rather than resolves the difficulties of his disparate subject by use of clichés and fancy terminology (for example, "problematics," when problems would have done just as well). On the whole, however, this book represents a massive achievement and a great advance over the brilliant pioneering work of Hugh Seton-Watson, *Eastern Europe between the Wars, 1918-1941* (London, 1945), and *Independent Eastern Europe: A History* (London, 1962) by C. A. Macartney and A. W. Palmer, which is marred by the blatant bias of its senior author.

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EMIL BIEDRZYCKI. *Historia Polaków na Bukowinie* [History of the Poles in Bukovina]. (Zeszyty Naukowe Uniwersytetu Jagiellońskiego, 300. Prace historyczne, 38.) Cracow: the University. 1973. Pp. 260. Zł 55.

The author, Emil Biedrzycki, is eminently qualified to write this history of Poles in Bukovina. A native of that region, he has long been a specialist

in Romanian literature and language and an interpreter of that culture to Poles at the University of Lwów and later at the Jagiellonian University.

This monograph belongs among those books dealing with closely defined regional and local studies, obviously written for the specialist of East Central Europe and historians of that phenomenon called nationalism. It is a work that treats with only one of the medley of national minorities inhabiting this area known by its historical name (since 1392) of Bukowina (Polish), Bucovina (Romanian), Buchenland (German), and Bukovyna (Ukrainian).

The role of Poles as colonizers in this area dates from the distant past and for some years the connection with Poland has been more than intimate. But it is obviously not the purpose of the author to write the full history of this relationship, as he devotes only eight pages to that period before the First Partition of Poland. He concentrates, rather, on that period between 1869 and 1918 when, under Austrian rule, Poles in Bukovina formed a compact community solidly conscious of its national roots. These feelings were especially strong in the urban area of Czerniowce.

Romanian and German historians have treated Polish contributions to the cultural and economic history of this area only marginally, while Ukrainians have, on the whole, been negative in their assessment. Biedrzycki, for his part, has a tendency to exaggerate the Polish legacy when he affirms the active role of the Poles, and the result frequently is a welter of names on a single page that only serves to distract the reader.

In view of the fact that Bukovina as an entity no longer exists (Northern Bukovina has been annexed by the Ukrainian SSR and Southern Bukovina is now part of Romania) and the Polish minority had long been dispersed, this fault (?) can be excused. Yet what the author intended, and has accomplished admirably, is to write a positive history of this minority before the archival material (scattered through Austria, USSR, and Romania) has perished. There is much of value here to scholars of East Central Europe as Biedrzycki has brought together a multitude of rich source material and given to the specialist an impressive piece of research on a topic of limited scope.

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JOZEF PILSUDSKI. *Year 1920 and Its Climax. Battle of Warsaw During the Polish-Soviet War 1919-1920. With the Addition of Soviet Marshal Tukhachevski's March Beyond the Vistula*. London: New York: Pilsudski Institute, 1972. Pp. 283.

This volume was conceived and published by the Pilsudski Institute as a centennial memorial to one

of the principal founders of the post-World War I Polish state. While the memorial is modest, the subject is superb: Pilsudski's finest hour—the victory of his forces over Soviet armies in the battle of Warsaw in August 1920, a triumph that has been called one of the decisive battles of the world. This description is quite accurate, for had they brought Poland under their control, Soviet armies might have gained Germany and the rest of Western Europe, which was then in the midst of a painful economic, political, social, and financial adjustment.

The volume consists of a lengthy introduction that sets the Polish-Soviet struggle in proper perspective; a brief analysis of that struggle by Pilsudski's adversary, Soviet Marshal M. I. Tukhachevsky, delivered early in February 1923, as a lecture at the Moscow Military Academy; and Pilsudski's lengthy response written between April and June 1924. As one might expect, the loser and the victor differ in their assessments of the outcome of the operation. Tukhachevsky attributes early Soviet successes to good reconnaissance, effective propaganda, division among Soviet adversaries, and foreign assistance (from Lithuania, Germany, and pro-Soviet groups in Western Europe), and imputes the final defeat of the operation (whose aim was to sovietize Europe) "not to political but to strategic factors" (p. 263), and to such additional elements as lack of technical equipment, poor communications, overextension, and Western assistance to Poland.

In response, Pilsudski corrects many of Tukhachevsky's facts, figures, and errors; criticizes him for intentional exaggerations or omissions; and presents his own version of Polish distribution of forces, tactical operations, reasons for advances as well as retreats, and numerous other clarifications of events that culminated in the "miracle along the Vistula," which, Pilsudski maintains, was prepared and executed by Poles in behalf of Poland. While Pilsudski's commentary is marred by frequent, but under the circumstances inevitable, elations over Soviet defeat and Polish triumph, the English translation of the battle of Warsaw will be welcomed by all students of military history.

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ANDRZEJ PILCH. *Studencki ruch polityczny w Polsce w latach 1932-1939* [The Student Political Movement in Poland, 1932-1939]. (Zeszyty Naukowe Uniwersytetu Jagiellońskiego, 299. Prace historyczne, 37.) Cracow: the University, 1972. Pp. 207. Zł. 33.

Students in interwar Poland, unlike those in America, were involved in lively, often violent, politics. Pilch's valuable monograph clearly illus-

trates their ideological pluralism even during the seven undemocratic years of prewar Poland. The first part chronologically presents the main political youth movements; the second deals with three issues: the students' stand in defense of academic autonomy against the encroachments of the *sanacja* regime, nationalistic conflicts which culminated in the tragic antisemitism led by the followers of Roman Dmowski, and the antituition struggle in the public schools of higher learning.

Like most works on recent history published in present-day Poland, Pilch's book somewhat distorts the true picture by its emphasis on the allegedly major role of the rather insignificant Communist student movement, which, according to his own figures, never exceeded 200 members, mostly of non-Polish ethnic origin (p. 67). Disregarding the unpopularity among all classes of prewar Poland of Marxism-Leninism and its Muscovite protectors, Pilch asserts that a substantial segment of the population in the Eastern half of Poland strove for unification of West Ukraine and West Byelorussia with the Soviet Republics (p. 150).

Though personally grateful for the author's recognition of this reviewer's role in organizing opposition to the fascistic and antisemitic trend among Lwów students, I can assure him that the "two" leaders of the PMSD (Polish Social Democratic Youth), Lerski and Sołtysik, are but one person since I then used the double name of Lerski-Sołtysik (pp. 125-126). Similarly, Rowmund Piłsudski is not a nephew, but a distant relative of Marshal Piłsudski (p. 32).

In view of his scrupulous listing of all possible Marxist publications and obscure Communist personalities, the author's omission of the seminal role played by such youth periodicals as *Polityka*, which that towering figure of the Polish emigration Jerzy Giedroyc (editor of Paris' *Kultura*) edited, and *Orka na Ugorze* ("Plowing the Fallow"), which the late Jaxa-Rozen and Herling-Grudziński edited, seems ominous. The same applies to his overlooking the important former leaders now active in exile: Gierat, Wierzbiański, Wilk, and Żenczykowski. Such shortcomings have a somewhat marring effect on an otherwise thoroughly researched contribution to explain the turmoil at Polish universities prior to the students' enthusiastic effort to defend their country in September 1939.

GEORGE J. LERSKI  
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BARBARA BOJARSKA. *Eksterminacja inteligencji polskiej na Pomorzu Gdańskim (Wrzesień-grudzień 1939)* [Extermination of the Polish Intelligentsia in the Gdańsk-Pomerania Region (September-December 1939)]. (Badania nad Okupacją Niemiecką w Polsce, number 12.) Poznań: Instytut Zachodni. 1972. Pp. 186. Zł. 35.

Nazi policies and practices in the occupied countries are probably the best-known chapters in the history of World War II. Records of thousands of war-crime trials, innumerable publications based on both scholarly research and personal experiences of survivors, and German secret archives, seized by the Allies and made available to the public, seemingly exhausted the subject. Still, thirty years since the end of the war, the influx of the authenticated data does not stop. It is as if humanity refuses to put its conscience at rest.

Barbara Bojarska limited her research territorially and concentrated on the relatively small area adjacent to the prewar Free City of Danzig. She also limited her study in scope by focusing on Nazi extermination of the Polish leadership: political, cultural, educational, religious, as well as economic. This extermination was not only a result of the corruption of the Nazi officials, but also represented the implementation of a long-range policy, formulated by Hitler himself. This policy aimed at reducing the Polish nation to the status of slaves in his Third Reich. "Only a nation deprived of its leading strata can be lowered to the rank of slaves," he stated. Already on September 12, 1939, while the Polish campaign was still going on, he issued pertinent orders to those of his subordinates who were to deal with the Poles.

The author demonstrates research in depth, undeviating methodology, careful verification of sources and statistical data, and avoidance of side-issues. Her work, if translated into English, would be appreciated by the international academic community. The book's revealing content notwithstanding, the reader will find in it a *vade mecum* which will acquaint him with not only the Polish literature on World War II but also the variety of archival material, both governmental and private, available in Poland for scholarly research.

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ULDIS ĢĒRMANIS. *Oberst Vācietis und die lettischen Schützen im Weltkrieg und in der Oktoberrevolution.* (Acta Universitatis Stockholmiensis: Stockholm Studies in History, 20.) Stockholm: Almqvist & Wiksell. 1974. Pp. 336.

The much awaited study of the Latvian *strelki* (which first appeared in Swedish as a thesis for a licentiate degree and was then serialized in a Latvian magazine in Canada) has finally appeared in German. The topic of this technically well-presented book is important for all students interested in the Russian Revolution in particular, and Russian history in general. Though the involvement of the Latvian *strelki* in the Russian Revolution is well known in Latvian and Soviet historiography, no book or article about them has ever appeared in a

Western language (with the exception of two chapters devoted to the topic in A. Ezergailis, *The 1917 Revolution in Latvia*).

The story of the *strelki* begins in the summer of 1915 when the high command decided to organize Latvian national troops for use in the Northern Front. The *strelki* began their association with the Bolsheviks soon after the March Revolution and continued it until the end of the Civil War in 1921. Ģērmanis' work, though, takes the story only to the Bolshevik uprising in 1917. He has promised to write a second volume covering the Civil War. In part the title of the work is misleading: though it purports to be a monograph on Colonel Jukums Vācietis (a commander of a *strelki* regiment and later the first commander of the Red Army), the author has very little to say about him. No substantive motivations for Vācietis' involvement with the Bolsheviks are provided. Moreover, the author failed to consult the lengthy autobiographical sketch that Vācietis wrote for the *Deiateli Sotuzs Sovetskikh Sotsialisticheskikh Respublik i Oktiabr'skoi Revoliutsii*. Because the author summarizes the events of October in such a schematic manner in the final chapter, he would have been better advised to leave that subject to the next volume. The main contribution of Ģērmanis' work is to summarize the secondary literature that exists in Latvian historiography on the *strelki*. Though the author has done a thorough job of researching memoir literature and some document collections, he has done little other work in primary sources.

Though this book (put in very respectable German by Andrejs Kubulins) is very important to Western scholars as an introductory text, it leaves much to be desired for experts who are acquainted with the original sources. One wishes that Ģērmanis' effort had surpassed the Soviet historian J. Kaimiņš' work *Latyshskye strelki v bor'be za pobedu Velikoi Oktiabr'skoi Revoliutsii*.

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TARAS HUNCZAK, editor. *Russian Imperialism from Ivan the Great to the Revolution*. With an introduction by HANS KOHN. New Brunswick: Rutgers University Press, 1974. Pp. xi, 396. \$17.50.

The articles in this collection—devoted to an examination of the motives, character, and results of Russian imperialism—are of uneven quality. The contributions of Henry Huttenbach and Walter Leitsch, concerned with Russian policies toward Ukraine and Poland respectively, avoid raising questions about the imperialist nature of these policies. The former does so by stressing, rather, the mutuality of religious and ethnocultural interests between the Ukrainians and the Russians in the seventeenth century and the role played by

some Ukrainians in integrating Ukraine into Russia; the latter does so by viewing Russo-Polish relations up to the late eighteenth century as essentially a clash between two rival empires, with the consequent annihilation of the one by the other. Neither deals with Russian policies in Poland and Ukraine in the nineteenth and twentieth centuries when Russian rule became synonymous with political, economic, and religio-cultural oppression.

Huttenbach, in a second article, again demonstrates his penchant for avoiding the question of imperialism by concerning himself solely with the origins—rather than with the motives—of Muscovite expansion. He limits the discussion of the consequences of this expansion for the subject nationalities of the empire to several superficial and baseless observations, offered almost as afterthoughts—that the “cultural and social autonomy allowed them [the Tatar and Mongol peoples] made the Muscovite yoke bearable.” An article by Ragnhild Hatton, on Russia's expansion to the Baltic from the late fifteenth to the early eighteenth centuries likewise avoids the issue of the imperialist nature of Russia's occupation of the eastern coast of the Baltic, although it mentions in passing Russia's commercial gains from involvement in the Baltic trade. The real value of her contribution, rather, is to be found in the detailed study she presents of the diplomacy and wars involving Russia, Poland, and Sweden for control of the Baltic Sea.

The essay by Emanuel Sarkisyanz, while admitting the existence of Russian imperialism, seeks to discount its deleterious effects on the non-Russian peoples of the empire by noting that other countries were equally imperialistic (an irrelevant point also made by Huttenbach in his article on Muscovy), that not all the non-Russian peoples were exploited to an equal degree, and that economics played a minor role in the “driving motivation” of Russian imperialism, all by way of noting that the true villain in the story is Stalin's “neo-Muscovite and totalitarian imperialism.”

An article by Traian Stoianovich, concerning Russia's involvement in the Balkans, is somewhat confusing. When he discusses Russia's policies there and in the Straits, he sees concrete political-diplomatic, military, and socioeconomic factors at work, the evidence for which constitutes the bulk of the article. But he then goes on to assert—in complete contrast to the prevailing spirit of the otherwise highly informative study—that “Russian domination of the Balkans was primarily ideological.” Only two articles, Firuz Kazemzadeh's “Russian Penetration of the Caucasus” and Geoffrey Wheeler's “Russian Conquest and Colonization of Central Asia,” deal directly and systematically with the subject at hand. Each



recounts the history of Russian military, political, economic, and religio-cultural penetration of the territories under discussion. Each discusses the techniques of direct and indirect control, the manipulation of local peoples and conditions in furtherance of this control, and the subsequent failure of local religio-nationalist elements to free their respective peoples from alien rule. For the peoples of the Caucasus, according to Kazemzadeh, this failure led to either physical extermination or mass deportation. In Central Asia, according to Wheeler, it resulted in the conditioning of the native peoples to submit to a new and presumably equally alien Bolshevik regime. An article by Taras Hunczak on pan-Slav ideology and one by Sung-Hwan Chang on Russian involvement in China and Korea round out the collection.

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L. E. SHEPELEV. *Aktsionernye kompanii v Rossii* [Joint Stock Companies in Russia]. (Akademiia Nauk SSSR. Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka." 1973. Pp. 346.

Although a definitive history of Russian capitalism has yet to be written, a substantial foundation has been built recently, particularly in the United States, France, and the Soviet Union. Many historians have focused on Russian capitalism's social, entrepreneurial, and institutional history. One may cite the works of I. F. Gindin, P. G. Ryndziunskii, and V. Ia. Laverychev, Roger Portal, Alexander Gerschenkron, and this reviewer. John McKay and René Girault have also treated at length foreign enterprise and investment in Russia. But much of the economic, political and cultural history of Russian capitalism remains to be elucidated.

Shepelev's study provides a unique Soviet contribution to this endeavor. His book is based on exhaustive research in Soviet archives. He has utilized the papers of the St. Petersburg Stock Exchange, as well as dozens of state agencies, industrial enterprises, and banks, and other documents. The author provides a wealth of detail that would be difficult for most Western scholars to come by. Shepelev's stated purpose is essentially descriptive. He explores a subject about which little has previously been written: the centralization of capital in industrializing Russia of the late Tsarist period and the economic policy toward stock companies of the imperial government and of its successor regimes.

The book's chapters can be divided into two groups: those that focus on quantitative aspects of

the stock companies' growth and those that deal with stock corporation and stock exchange regulation.

The first Russian stock corporation, established in 1755, was a diving company operating in the Gulf of Finland. Other stock companies appeared in the early nineteenth century, but the tempo of growth did not accelerate substantially until after the Crimean War, with the founding of hundreds of companies each decade, and then in the boom of the 1890s, when hundreds appeared each year. The early companies were involved with commerce, insurance, textile manufacturing, and food processing, but by the 1870s, banks and railroads had achieved primary importance. By 1914, Russia had 2,263 stock corporations with a capital of over 4.5 billion rubles, compared to 5,488 in Germany, amassing the equivalent of over 8 billion rubles.

The 1890s also witnessed the growth of a large-scale speculative stock market in Russia. An outgrowth of the merchants' exchanges of the early nineteenth century, a sophisticated stock exchange, similar to those of London and Wall Street, had developed in St. Petersburg by the end of the 1890s, with twenty other active exchanges in various cities of the empire. With the stock exchange came the scenes of speculative activity, so familiar in Europe and America.

Nicholas I and Count Kankrin were suspicious of stock companies, which they saw as nests of dishonest speculation. Yet they wished to stimulate industrial growth. This was reflected in the first legislation of 1836, which affirmed the institution of the stock corporation and the limited liability of shareholders, but sought rigorous government control. This legislative and administrative surveillance continued throughout the Tsarist period, and much of Shepelev's book is devoted to an analysis of the most important projects and laws.

The climactic episode of the history of Russian stock companies came in 1917 and 1918, and is briefly described in the final chapters. There was an outburst of financial activity during the period of the Provisional Government, during which 734 stock companies with a capital of almost 2 billion rubles were formed, twice to quadruple the level of stock-company formation in 1913. Beginning with the decree of December 14, 1917, the Soviet government began the seizure of large stock corporations and confiscation of all capital which earned more than 500 rubles per month. Within a year, the whole system had been destroyed.

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UWE LISZKOWSKI, editor. *Russland und Deutschland*. (Kieler historische Studien, number 22.) Stuttgart: Ernst Klett Verlag. 1974. Pp. 334. DM 98.



This *Festschrift* was dedicated to Georg von Rauch on his seventieth birthday. Von Rauch is one of the Federal Republic's leading specialists on the Baltic area and Russia from the seventeenth to the twentieth century. Though best known in this country for the English version of his short history of Soviet Russia, von Rauch's wide-ranging scholarship has touched numerous aspects of pre-1917 history as well. The theme of German-Russian interactions and perceptions provides a common thread for the contributions to this volume.

Many of the pieces are of minor interest. Worth noting are Günter Wiegand's report on Eastern Europe in the late-medieval chronicles of Lübeck; Norbert Angermann's report on German merchants in the trade of seventeenth-century Pskov; and Erik Amburger's discussion of the Wogau-Konzern's role in the economy of Moscow and Russia from the early nineteenth century to 1917. There is food for thought in Klaus Zernack's review of "negative policy toward Poland" in German-Russian diplomacy in the eighteenth century. Michael Garleff and the editor, Uwe Liszkowski, deal with interesting aspects of the change from a positive to a negative perception of Poland in late nineteenth- and early twentieth-century Germany and Russia. The relations of radical political parties are usefully represented by Wolfgang Geierhos' discussion of the German Social Democratic Party's attitude toward Russian revolutionaries in the early 1880s and by Albrecht Buchholtz's sidelights on the career of the important Soviet diplomat, Leonid Krassin.

All these, like the twelve other contributions, a short biography of von Rauch, a list of his publications, and a register of the dissertations prepared under his direction, will be assured a decent burial by the price of the book.

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V. V. KABANOV. *Oktiabr' skaya revoliutsiia i kooperatsiia (1917 g. - mart 1919 g.)* [The October Revolution and Cooperative Societies (1917–March 1919)]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 293.

Although the October Revolution is generally associated with discontinuity in Russian history, the development of the USSR can be fully understood only if one takes into account the important social and cultural institutions which survived. One of the more significant and neglected of these was the cooperative movement—the producers', consumers', and credit institutions which developed rapidly in the decade before 1917—and came at the height of War Communism to serve some 100 million people through more than 53,000 separate as-

sociations. This small but detailed volume documents the cooperatives' incorporation into the Soviet state system and argues their importance, despite "bourgeois origins," as institutional and cultural precursors of Soviet socialism.

The author has a broad and sympathetic vision. He sees the cooperatives before October as instruments in the struggle against monopoly capitalism and argues they cultivated both an attitude of cooperation among workers and peasants and a spirit of economic independence. Appreciating the administrative expertise of their staffs, he insists on the invaluable service they performed during the Civil War. One gains an insight into the extensiveness of cooperative operations, the humanitarian, non-political commitment of their administrative personnel (many of whom continued to work for the new government), and the complex process by which the Bolsheviks absorbed their distribution and credit apparatus.

This latter process is the author's central focus. After detailed reviews of historiography and sources, he concentrates on cooperative activity in the immediate post-October period and on the Bolshevik policy of "compromise" in the spring of 1918. He then examines the party's "struggle for control," presenting a variety of statistics, noting the problem of political dissent, and discussing the struggle between cooperatives and groups like the committees of poor peasants (*kombedy*). His emphasis is on the consumers' cooperatives, the most extensive cooperative form. But he examines as well the role of cooperative credit institutions, arguing that the level of deposits increased between 1917 and 1918, "evidencing the trust of the petty bourgeois in the proletarian state" (p. 135). Most interesting are figures of overall cooperative growth in this period and the indication they give of the enormous role these institutions played in distributing goods and providing urgently needed funds for production.

Only 1700 copies of this book were published, but it is an informative and suggestive work. Some may be bothered by the author's inflexible class categories, his failure to penetrate the inner workings of cooperatives, his neglect of producers' associations, his failure to follow closely the fate of cooperative staffs, and his excessive concern for Lenin's views. But those interested in either the cooperatives themselves or the process of institutional transformation after October will find it serious, scholarly, and useful. Of special note is the inclusion of an index, rare in Soviet books.

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A. AVTORKHANOV. *Proiskhozhdenie partokratii* [The Origin of Partocracy]. Volume 1, *TsK i Lenin* [The

Central Committee and Lenin]; volume 2, *TsK i Stalin* [The Central Committee and Stalin]. Frankfurt/Main: Possev-Verlag. 1973. Pp. 728; 534. DM 32; DM 23.

In this mammoth narrative of the Bolshevik leadership in Russia, Abdurakhman Avtorkhanov has analyzed the nature of the Soviet regime and the contributions made by Lenin and Stalin, to whom the two volumes are respectively devoted, in creating the system of totalitarian rule that has persisted since the Great Purge.

Serving for the past two decades as a political analyst for Radio Liberty and the Institute for the Study of the USSR in Munich, Avtorkhanov brings the perspective of an eyewitness to these events, for he was a Communist party functionary in the thirties until he became a victim himself of the Great Purge. (He was released from prison in 1942 only to be captured by the Germans shortly thereafter.) His previous works, both drawing on this experience, include *The Reign of Stalin* (1953), under the pen name of Alexander Uralov, focusing on the purges, and *Stalin and the Soviet Communist Party* (1959), centering on Stalin's fight with the Right Opposition and his consolidation of power up to the purges.

Avtorkhanov's stated aims are to explain Lenin's political role, the nature of the post-Lenin succession struggle, and the origins of Stalin's "criminal Bolshevism." In detailing these developments he provides a useful congress-by-congress guide to the debates between the party leadership and the long sequence of opposition factions up to the point when Stalin put an end to all meaningful discussion.

Avtorkhanov credits Lenin, thanks to his "will to personal power" (1:610) and his use of opportunists like Stalin to curb the idealists in the party, with laying down the basic principles of the "partocracy"—"a hierarchical system of absolute political, economic, and ideological power and rule of the party within the party—the apparatus of the CPSU" (1:31). On the other hand, he holds Stalin responsible for a well-planned campaign to destroy the Leninist old guard and the collectively led party in favor of a new system of personalistic terror. A "Leninist more than Lenin himself," Stalin carried the principle of "partocracy" to its logical extreme and to the "ruin of the Leninist Central Committee" (1:62). "That which Lenin started off in embryo," Avtorkhanov observes, "Stalin raised up as a monster" (2:477).

The most interesting portion of Avtorkhanov's work is his detailed description of Stalin's struggle to consolidate his power against the Right Opposition led by Bukharin. This, of course, is the period where the author's account begins to be enlivened

with his observations as a lower-level participant in the actual events. Fortunately, this material appears in his works that were previously published in English.

Avtorkhanov makes few factual slips for a work of this scope. It was Stalin, not Lenin, who said "Marxism is not a dogma but a guide to action" (2:158); the 1927 Platform of the Fifteen was issued not by the Trotskyists but by the separate Democratic Centralist group. Certain statements by the author will be controversial. He alleges that Lenin engineered the assassination of the German ambassador, Count Wilhelm von Mirbach, in 1918 as an excuse to suppress the Left Socialist Revolutionaries (1:506–67). In the maneuvers to suppress Lenin's "Testament" in 1923–24, Avtorkhanov contends that Stalin falsified Lenin's instructions to excuse withholding the document until after Lenin's death (2:47–52).

A much broader question of interpretation is raised by Avtorkhanov's view of Stalin as an almost superhuman political strategist, shrewdly calculating his moves years ahead, according to his "principle of unprincipledness" (2:384), in pursuit of a preconceived goal of total personal power. Such a view is retrospectively persuasive, as many biographical works since Trotsky's *Stalin* testify. It underrates, however, the unpredictable, unfolding nature of politics anywhere and the unforeseeable good fortune Stalin enjoyed in the political naïveté and disunity of his opponents. It also leaves unanswered the question of the extent to which Stalin's principles of "partocracy" are still imbedded in the Soviet system of rule.

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ALBERT S. LINDEMANN. *The 'Red Years': European Socialism versus Bolshevism, 1919–1921*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xviii, 349. \$15.75.

*The 'Red Years'* is an ambitious undertaking aimed at providing an overview and synthesis of the history of the relationship between the three major Western European socialist parties (German, French, and Italian) and the Bolsheviks between 1919 and 1921.

Given this objective, the book provides not only a great deal of specific information on the connection between the parties but also about larger issues. The latter include how the relationship between Western European socialists and the Bolsheviks influenced the development of the Soviet regime, how the October Revolution affected the socialist parties of Western Europe, and, most important, how in turn the demise of revolutionary hopes in Western Europe shaped Lenin's policies.

While the October Revolution provided inspiration and enthusiasm for socialists in Western Europe, its ambiguities and peculiarly Russian characteristics did not make it easy for Western socialists to develop policies helpful for the cause of the proletarian revolution in their own countries. As the author makes clear, "... the bolsheviks had not made a proletarian socialist revolution in any full or Marxian sense of the term. The only plausible rationale for their takeover in Russia was that they were starting something that necessarily had to be completed in the highly industrialized West, where the material conditions existed upon which to build socialism. ... They were able to hold power not so much because of the strength of their proletarian backing, but because opposition to them was divided. Russian society was so atomized that a ruthless and terroristic minority could maintain power over it. This was not an auspicious beginning to world socialist revolution" (p. 33).

Deviating somewhat from the conventional assessments of the role of the Bolsheviks, the author also stresses that World War I brought to the surface the schisms and factionalism among Western socialists. These were, in the final analysis, decisive factors in obstructing the development of strong, united socialist movements which could have had a revolutionary potential. With appropriate detachment, Lindemann concludes that the conflict between Western socialists and the Bolsheviks produced "few unsullied heroes," and that the postwar divisions among Western socialists had deep historical roots independent of Bolshevik policies.

The valuable contributions of this work, besides documenting and elaborating its main thesis, include the detailed portrayal of the personalities and policies of various Western socialist leaders, a reassessment of Lenin's attitudes toward Western socialism and world revolution, and the examination of the social and historical conditions which created the particular characteristics of each major Western European socialist party and their various factions. (The author distinguishes among "sycophants," "syndicalists," "opportunists," "independents," and "old Marxists" in regard to attitudes toward the Bolsheviks.)

Given the scope and complexity of the topic and the author's determination to weave together many divergent historical threads, it should not be surprising if the reader is at times left with the impression that certain subtopics could have been more fully discussed and the book as a whole better focused.

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*Kritika burzhuaznoi istoriografii Sovetskogo obshchestva* [A Criticism of Bourgeois Historiography of Soviet Society]. Moscow: Izdatel'stvo "Politicheskaya Literatura." 1972. Pp. 411.

This volume of seven chapters, written by nine authors, analyzes the writings of Western scholars, public figures, and journalists about major problems of Soviet history, ranging from the October Revolution to collectivization, the Second World War, and the Soviet post-war era. To a large degree the views and conclusions expressed here will come as no surprise to those already familiar with Soviet impatience with foreign criticism. The footnote citations of Western literature show broad acquaintance with journal literature, memoirs of public figures, and scholarly monographs, but this is also characteristic of much Soviet writing on other, less sensitive topics. While there is a reduction in the use of the harsher terms of the Soviet lexicon such as "falsification" to describe Western views, there are still enough examples to make this a dreary volume to read. In addition, while the West is sharply criticized for its errors of judgment about the Soviet situation, there is an equally extreme approval of the acts and opinions of the USSR.

Nevertheless, Western analyses have, indeed, had their flaws of haste, prejudice, and incomplete use of sources, and at times the authors of this volume have made some telling points. Trying to find them, however, in a book that lacks index and bibliography, and that is, one feels, so insufferably sure of everything as the rest of us wish we were of anything, is perhaps not worth the labor.

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IWAN KOSZELIWEK. *Mykola Skrypnyk*. New York: Suchasnist Publishers. 1972. Pp. 342.

MYKOLA SKRYPNYK. *Statti i promovy z natsional'noho pytannia*. [Articles and Speeches on the Nationality Question]. Compiled by IWAN KOSZELIWEK. Munich: Suchasnist Publishers. 1974. Pp. 370.

Skrypnyk, an old Bolshevik and close associate of Lenin, was to a great degree responsible for the formulation of the Soviet nationality policy in the 1920s, for the creation of the federative structure of the USSR, for the establishment of the bicameral legislative assembly of the Soviet Union and for the compromise solution of preserving the unity of the Party, but with territorial subdivisions coinciding with the boundaries of the constituent republics.

Skrypnyk had been a ruthless member of the Cheka, and an effective prosecutor in the Ukraine. But he was also firmly convinced—as the result of Bolshevik defeats in the Ukraine—that the Com-

munist regime could survive in territories which were not ethnically Great Russian only if it identified itself with the national and social strivings of the minorities. Fearful of the reactionary tendencies of the members of the Tsarist bureaucracy, many of whom remained in administrative positions during the Bolshevik regime, Skrypnyk argued against the "petit bourgeois" characteristics of the various shades of "Russian imperialists" inside and outside the Communist party. When he became responsible for the educational network in the Ukraine, Skrypnyk started to implement the policy of Ukrainization, trained Party cadres and encouraged, with some success, emigré Ukrainians to return. These policies, which he was always careful to label, with some justification, as Leninist, brought Skrypnyk into conflict with Stalin. Skrypnyk, however, allegedly was not familiar with or chose to disregard Lenin's privately expressed sentiments vis-à-vis the national minorities. In the power struggle which took place after Lenin's death, Stalin won by playing faction against faction. But Skrypnyk, by committing suicide in 1933, foiled plans for a larger show trial which Stalin was preparing. Koszeliwec argues, admittedly with no direct evidence, that Skrypnyk committed suicide as a sign of protest against his own policies. Having become a disillusioned Marxist, Skrypnyk saw suicide not only as a way out, but also as a telling argument against himself.

In the destruction of millions of Ukrainian peasants and thousands of members of the intelligentsia, including the leading members of the Communist party, which followed in the wake of his suicide, Skrypnyk himself became a posthumous enemy of the people. Although he has been officially rehabilitated after de-Stalinization, little has been written about him in the Soviet Union. The Koszeliwec study, based on published sources and a few personal interviews, at times calls the reader's attention to the contemporary situation in the USSR, whenever such parallels seem warranted. But despite the author's comment that his writing might be considered polemical, the study on Skrypnyk is generally free of such overtones. Moreover, the book includes a full bibliography of Skrypnyk's works and an autobiographical sketch.

The companion volume of Skrypnyk's speeches and articles, edited by Koszeliwec, is useful because the material has not been readily available. Skrypnyk was an extremely prolific writer, and Koszeliwec wisely limited the collection to Skrypnyk's contributions on the nationality question. It is a crucial, and still largely overlooked, issue in the study of the USSR and one not likely to be tackled by official Soviet scholars. Moreover,

Skrypnyk is at his oratorical and polemical best when he deals with the nationality question.

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EDUARD WINTER. *Russland und das Papsttum*. Part 3, *Die Sowjetunion und der Vatikan*. (Akademie der Wissenschaften der DDR, Zentralinstitut für Geschichte, Quellen und Studien zur Geschichte Osteuropas, volume 6.) Berlin: Akademie-Verlag. 1972. Pp. x, 338.

This is the final volume of Eduard Winter's history of relations between Russia and the Papacy from the conversion of the East Slavs to the Soviet period.

Published under the auspices of the Academy of Sciences of the German Democratic Republic, this volume is understandingly inimical to the Vatican. It is worth reading, however, because of the wealth of research materials used by the author, especially the publications by the Holy See and other Catholic institutions, German, Czechoslovak, and Soviet diplomatic documents, various learned journals and pertinent books. Whatever the interpretation, facts are truthfully reported.

Winter sees the Vatican's attitude toward Soviet Russia as guided by hostility toward Communism and the wish to achieve a union with the Russian Greek Orthodox Church. He does not conceal Soviet persecutions (they are not persecutions for him) of Catholics in the Soviet Union, but stresses the resulting enmity of the Vatican. According to Winter, Pius XII considered the USSR as the Vatican's main enemy, while Nazi Germany was the lesser evil. Interestingly enough, while he describes the horrors of Nazi occupation of Eastern Europe and the Soviet Union, he never mentions the Jews.

John XXIII is the only pope whom he praises, because of his conciliatory attitude toward the Soviet Union. Paul VI's policy is characterized as hesitant and contradictory, though he is searching for a *modus vivendi* with the Soviet Union.

The weakness of the book is the author's neglect of the problems of the Church in Eastern Europe, notably in predominantly Catholic Poland, where Paul VI strives to achieve a *modus vivendi* between the Church and the Communist government, often to the displeasure of the Polish hierarchy.

The author is right, however, in arguing that recently both the Soviet government and the Vatican have followed conciliatory policies. Official visits to the Vatican by Foreign Minister A. Gromyko (1965 and 1966) and the chairman of the Presidium of the Supreme Soviet, N. Podgornyi (1967), as well as by representatives of the Russian Greek Orthodox Church (1967, 1968, and 1969)



provide the evidence that Moscow sees better relations with the Vatican as part of its détente policy, while the Vatican hopes to alleviate the situation of Catholics in the Soviet Union, particularly in Lithuania and to reach better relations with the Greek Orthodox Church in the ecumenical spirit.

A historian interested in the Vatican's foreign policy would be well advised to include this book in his research materials.

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M. LEWIN. *Russian Peasants and Soviet Power: A Study of Collectivization*. Translated by IRENE NOVE with the assistance of JOHN BIGGART. With a preface by ALEC NOVE. (The Norton Library.) New York: W. W. Norton and Company. 1975. Pp. 539. \$4.95.

This is an intelligent, lively, and in some respects persuasive book concerning the political, social, and economic background of the Soviet "great turn" toward massive collectivization of peasant agriculture, initiated in the USSR in 1929. On a number of issues—notably on the political conflicts among the left, right, and center factions of the Communist party, which started in 1923/24 and culminated in 1928 with the crushing of the left and right factions—Lewin goes over well-worn terrain. The main interest of this book is in the suggestion that collectivization was the "culmination of a chain reaction" set off by the 1928 crisis in state procurement of agricultural deliveries from the peasants, and that the result of Stalin's forceful, "emergency" solution of this crisis consolidated his own personal power under the form of the "Stalinist totalitarian dictatorship" (pp. 516–17).

The long-standing conflict among the party's factions concerned *precisely* the twin issues of how to industrialize (that is what priorities should be set and what pace should be selected) and what to do with the peasants and private agriculture (that is when, how, and to what extent should agriculture be tapped in order to fuel industrialization). The procurement crisis may or may not have triggered the collectivization drive, but the question had been "in the air." Indeed, the establishment of market relations at the periphery of the state complex, and the acceptance of a privately owned agriculture, inaugurated in 1921 under the name of New Economic Policy (NEP), was not considered by many party leaders as a *permanent* fixture. It was rather a *retreat to be overcome*. Undoubtedly, however, once the collectivization started, Stalin pushed it heedlessly and brutally to its end.

Certainly Stalin's dictatorship came out strengthened from this process. But Lewin entertains disarmingly naive ideas as to what the party

was *before* Stalin fastened his own grip and his own "personality cult." According to Lewin, because of the 1929 turn "the principle of personal loyalty came to take the place of programme, criteria, and analysis, and . . . a Marxist, rationalist, revolutionary Party ended up, by a peculiar twist, with a personality cult" (p. 452). This is also tied to the idea that the party became increasingly bureaucratic, losing as it were its pristine revolutionary character.

Actually, the "bureaucratization" of the party had started from the very first day it grasped power. A highly centralized machine, intensely committed to Lenin's leadership and accustomed to operate through secret committees, the party and the soviets spawned immediately committees and subcommittees completely removed from any "mass" control. Certainly, Lenin was more modest and less crude, though not less brutal with his adversaries; but he ran the party machine in the same highly personal fashion as Stalin, who only accented certain features. These were part and parcel of the party's structure, of its methods, and of its basic policy of imposing its will on the state and on the economy.

Lewin is particularly successful in describing the situation and the mood in the countryside on the eve of the "great turn." He has not taken full advantage, however, of the relatively abundant statistical data available on the socioeconomic structure of the countryside. The *Statistical Yearbook of the USSR for 1928* (Moscow, 1929) for instance, contains some 218 pages on various interesting samples concerning the villages. Through an inspection of these tables, one can readily visualize the smallness of the peasant holdings, the limitation of available capital, the arbitrariness of the division into "poor," "medium," and "wealthy" farmers, and the unbelievable poverty out of which the party squeezed the wherewithal for the great industrialization drive.

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K. ROKOSHOVSKY. *A Soldier's Duty*. Translated from the Russian by VLADIMIR TALMY. Edited by ROBERT DAGLISH. Moscow: Progress Publishers. 1970. Pp. 340.

A. EREMENKO. *The Arduous Beginning*. Translated from the Russian by VIC SCHNEIERSON. Moscow: Progress Publishers. 1966. Pp. 328.

K. A. MERETSKOV. *City Invincible*. Moscow: Progress Publishers. 1970. Pp. 94.

K. A. MERETSKOV. *Serving the People*. Translated from the Russian by DAVID FIDLON. Moscow: Progress Publishers. 1971. Pp. 377.



S. M. SHTEMENKO. *The Soviet General Staff at War, 1941-1945*. Translated from the Russian by ROBERT DAGLISH. Moscow: Progress Publishers. 1970. Pp. 399.

ANDREI GRECHKO. *Battle for the Caucasus*. Translated from the Russian by DAVID FIDLO. Moscow: Progress Publishers. 1971. Pp. 367.

M. V. ZAKHAROV, editor. *Finale: A Retrospective Review of Imperialist Japan's Defeat in 1945*. Translated from the Russian by DAVID SKVIRSKY. Moscow: Progress Publishers. 1972. Pp. 247.

I. S. KONEV. *The Great March of Liberation*. Translated from the Russian by DAVID FIDLO. Moscow: Progress Publishers. 1972. Pp. 286.

A. VASSILEVSKY et al. *Moscow 1941-1942 Stalingrad: Recollections, Stories, Reports*. Compiled by VLADIMIR SEVRUK. Edited by BRYAN BEAN. Moscow: Progress Publishers. 1970.

MANFRED KEHRIG. *Stalingrad, Analyse und Dokumentation einer Schlacht*. Stuttgart: Deutsche Verlags-Anstalt. 1974. Pp. 680.

JÜRGEN FÖRSTER. *Stalingrad Risse im Bündnis, 1942-43*. Freiburg: Verlag Rombach. 1975. Pp. 172.

The many armies that returned home in 1945 marched directly into print, where they have remained since World War II became the most written-about war of all time. Two of the main participants, however, the Soviet Union and Germany, joined the march late. Although the Soviet Army played a major role from 1941 on, nothing of consequence was published while Stalin lived, in part owing to the personality cult and in part to an exaggerated security consciousness. Khrushchev eliminated the first obstacle in his 1956 Twentieth Party Congress speech and at least mitigated the second. The wave of publication that followed was capped by the six-volume *Istoriia Velikoi Otechestvennoi Voyny Sovetskogo Soiuza*. (Moscow, 1960-63). Since the middle 1960s, the Brezhnev-Kosygin regime has allowed the Khrushchev wave to become a flood in volume of print but a mere ripple in terms of new information. While the German Federal Republic did not restrict or control publication of World War II history, German historians lacked access to records until the early 1960s when the captured military documents held in England and the United States were returned to Germany. War history research also had to do without official support before the *Bundeswehr* established the Military History Research Office in the early 1960s. The works considered here represent some of the recent Soviet and German output.

Of the Soviet works, four are memoirs, currently a popular form in the Soviet Union. All were published after 1965. By then, most western generals of memoirist caliber had long before banked their

advances and collected their royalties. But the delay was not necessarily a disadvantage for the Soviet generals. Among the present four, only Rokossovsky had "a name" during the war. The others did not achieve real stature until after Stalin died. For the Soviet government the belated publication has, moreover, a positive advantage: information that would have been made public elsewhere twenty-five years or more ago can now be presented as "new."

Out of date by Western standards, the memoirs are nevertheless important to current Soviet World War II historiography. Besides recognizing individual military prominence attained during the war or after, they allow apparently open discussion on a conveniently compartmentalized basis. Each author is free to confess his own shortcomings, which few generals anywhere are inclined to do in excess, and praise or blame his subordinates. He can also take occasional issue with his superiors but only on matters specifically related to his own command. Consequently, the generals write extended vignettes—Eremenko on Mogilev, Smolensk, Briansk, and the Fourth Shock Army; Meretskov on Karelia, the Volkhov, Karelia again, and the Far East; Rokossovsky on a succession of corps, army, and army group commands up and down the front. Shtemenko offers a novelty, a veiled but, particularly in the latter chapters, instructive picture (the only one extant) of the workings of the General Staff.

Not only writers of memoirs, the generals are, for several reasons, also historians. For one, the war is still too important an event in the Soviet Union to be left to mere academicians. For another, not every major figure had a role in the war commensurate with his status in the 1960s. Then too, in the Soviet Union the past must be kept in tune with the present; and, lastly, no doubt there is just the simple pleasure of authorship. Marshal Grechko, for instance, finished the war in command of the First Guards Army, not an unimportant post, but by 1967 he was Minister of Defense of the Soviet Union. In *Battle for the Caucasus* he deals with a whole theater of the war—as a historian rather than as a memoirist, although he commanded armies there. Actually the fighting in the Caucasus was not crucial in the war, but Colonel L. I. Brezhnev was there as a political officer. In Grechko's hands the Caucasus campaign emerges as a model of military operations with implications for the present and the future. *Finale*, edited by Marshal Zakharov, who was Chief of Staff of the Trans-Baikal Front during the war, performs a similar service for the August 1945 offensive against the Japanese in Manchuria. The contributors—one marshal (Malinovsky), two major generals, and three colonels—conclude that the oper-

ation against the Kwantung Army was "the most devastating defeat suffered by the Japanese Army during the Second World War" and that "this was the decisive factor in bringing about Japan's collapse and hastening her final defeat and unconditional surrender." On the atomic bomb they cite a Soviet-sponsored "Japanese" history of the Pacific war which states, "Even the atomic bomb had no effect on [Japanese] state policy as laid down by the Supreme War Council." It is of passing interest that this statement, frequently made in Soviet publications, is always attributed to "Japanese historians." *The Great March of Liberation* features three marshals—Grechko, Konev, and Zakharov—two colonel generals—Sharokin and Zheltov—and a lieutenant general—Telegin—writing on the 1944-45 campaigns in east-central Europe and the Balkans.

*Moscow-Stalingrad* is an anthology published to commemorate the twenty-fifth anniversary of the victory in Europe. The fifteen selections include memoirs of Marshals Rokossovsky, Vassilevsky, and Zhukov. The rest are a mixture of fiction and reportage by such writers as Simonov, Bergholtz, and Yuri Zhukov. The quality of the writing, translations, and illustrations is markedly higher than in the other books, but, except possibly for color, its usefulness as history is slight.

The two German works are productions of the *Bundeswehr's* Military History Research Office and reflect its approach to World War II history. Both are parts of series that will not, as far as can be seen, add up to comprehensive war histories in the way that the U.S., British, and Soviet official series do. They are elaborately documented, astutely objective, detailed analyses of meticulously excised parts of the war. The authors, apparently have seen and made note of every available document and account in German or Russian, ignoring works in other languages.

Kehrig's *Stalingrad* is a logistical and tactical study of the battle from October 6, 1942, to February 2, 1943, in 545 pages of text with 68 (93 pp.) documentary appendices. Although it is the most extensive and intensive work on the subject to date, it stops without a conclusion at the collapse of the XI Corps on the morning of February 2. The Foreword promises "a historical evaluation . . . when the fundamentals have been mastered."

In the effort to come to terms with the meaning of the defeat, Förster's *Stalingrad* investigates the effects on Germany's allies—Italy, Romania, and Hungary—and on neutral Turkey. Passing lightly over the defeats the allied armies suffered in and around Stalingrad, Förster deals with the friction with the allies, peace feelers, the projected Balkan alliance with Turkey, and Hitler's attempts to

patch up relations in the conferences at the Berghof and Schloss Klessheim in April 1943.

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M. M. KRAVETS'. *Ivan Franko—Istoriya Ukraïny* [Ivan Franko—Historian of the Ukraine]. L'viv: Vydavnytstvo L'vivskoho Universytetu. 1971. Pp. 202.

Ivan Franko (1856–1916), the great Ukrainian poet, was a noted scholar of the history of Ukrainian literature and a writer whose fictional works are not without value for the study of Ukrainian history. This volume, however, is the first to recognize Franko as a historian, though he never presented himself as such and had no formal training in the field. The author, a professor of history at L'viv University, discusses in five chapters Franko's approach to history, his research on feudalism in Ukrainian history, his study of the Ukrainian people in the capitalist epoch, his views on Ukrainian historiography, and, finally, Franko as an editor of Ukrainian historical sources.

In the opening chapter, Kravets' attempts to prove that Franko based his writings on the works of Marx and Engels—with a few admitted deviations (p. 14). He erroneously derives this conclusion from Franko's translation of one chapter of *Kapital* into Ukrainian and from undocumented claims, for example, that Franko saw Ukrainian national aspirations as part of the class struggle (p. 16). True, Franko as a humanitarian was an internationalist, and as a Ukrainian living under Austro-Hungarian rule, he was a revolutionary democrat. Yet in his autobiography, *V Poti Chola* [In the Sweat of the Brow] (1890), Franko wrote of himself at age 21: "I was a socialist merely by sympathy like any peasant, but far from understanding what scientific socialism was." From the same source comes: "I never belonged to that sect of the faithful who founded their socialistic program on . . . class warfare." Dr. Okhrymovych, a colleague and Marxist enthusiast, wrote of the poet at middle age: "Franko took a critical attitude toward Drahomaniv and Marxism and tended to revisionism and Fabianism" (see Percival Cundy, *Ivan Franko* [1948], p. 39).

In the chapters that follow, the author finds Franko's writings essentially in accord with Soviet views and, despite Hrushevsky's studies, the most profound treatment of pre-Soviet Ukrainian history. Kravets', a historian of recent Ukrainian labor and peasant history, while lauding Franko's efforts, notes errors in his writings on the "feudal" and "capitalist" eras and in his editing of ancient historical sources. (Interestingly, Franko did not even allude to feudalism, much less use the word,

in his writings.) In the chapter on Ukrainian historiography Kravets' attempts to show Franko as a historian in dispute with well-known Ukrainian historians, such as Hrushevsky, but the points raised by Kravets' are more or less insignificant (e.g., nonutilization of a journal, p. 181). Finally, though Kravets' could hardly be expected to go against the line of Soviet Academician Pashkov, his undocumented assertion that Franko saw "the bright future of his people" as "union with the Russian people" is simply false. Among other proofs Franko's poem, "Ne Pora" (1880), alone refutes this: "No longer, no longer should we the Russian or Pole meekly serve! . . . Let love be for the Ukraine alone."

As trying as publishing in the USSR of the history of non-Russian peoples is, Kravets' deserves credit for this book. Perhaps the curiosity he will raise among his Ukrainian readers about certain controversies in that republic's past will be merit enough.

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#### AFRICA

NORMAN R. BENNETT. *Africa and Europe from Roman Times to the Present*. New York: Africana Publishing Company, 1975. Pp. 246. Cloth \$12.50, paper \$5.95.

To compile a coherent history of Africa from Roman times to the present is a massive task; to complete such a work in one usable survey would seem to be impossible. Yet Norman R. Bennett has written such a work and has made an excellent contribution to African history. According to Bennett, his purpose was to use the most recent scholarship to present a short account of the relationship between Africa and Europe over several thousand years of history. He has not only fulfilled this purpose, but has gone beyond it to shed light on recent African developments.

In a work of this sweep, the reviewer naturally looks for basic errors in fact. That there are surprisingly few reflects the depth of Bennett's scholarship and research. He has taken the threads of both North and sub-Saharan African history and intertwined them into a reasonable fabric. At times it seems that the portion pertaining to the area south of the Sahara is stronger than that to the Maghrib. This, of course, seems to reflect Bennett's interests. The brief analysis of British and French colonial policy is an excellent summary of the two systems; the chapter, "African Responses," which outlines the often neglected African reaction to the European colonizer, is also valuable.

Free from nagging errors, well written, the product of the best of recent research, *Africa and Europe*

should serve the needs of those wanting a succinct overview of African history.

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VICTOR N. LOW. *Three Nigerian Emirates: A Study in Oral History*. Evanston: Northwestern University Press, 1972. Pp. xv, 296. \$15.00.

The history and development of state systems in the Western Sudan has generally focused on the "greats." For centuries the Hausa States—Bornu, Wadai, and Dafur—were the polities best known to outsiders. Monographs on Jukun and Nupe have placed these lesser polities into the literature. Nevertheless, this leaves a plethora of smaller kingdoms out of the comparative roster. Low has chosen to do this job for the so-called nineteenth-century border states of Gombe, Katagum, and Hadejia, which lay between Bornu on the east and the rising expansionist power of the Sokoto Caliphate on the west. These states were induced into existence by the Fulani *jihad*, and throughout the nineteenth century they referred to Sokoto as the ultimate authority: "The Emirs and their successors continued periodically to furnish Sokoto with martial aid and a share of state revenue and attended on the Sultan whenever summoned" (p. 12).

After summarizing the political organization of these states, the author discusses his methods and launches a historical account of the border emirates divided into early and later nineteenth century. The author also attempts in a major way to reconstruct the population sizes, ethnic distributions, fief holdings, and boundaries as these were about 1900 when the British took over the area.

There is no other book that covers this subject or puts together the archival materials with local oral traditions. In this sense Victor N. Low's work is certainly a contribution. Like it or dislike it, scholars will be forced to consult it. Low's understanding, however, of the kind of data necessary to document political history in the Sudan seems extremely limited. Why, or even how, Gombe, Katagum, and Hadejia have differentiated as political systems from the accounts and models provided by Smith, Nadel, and others escapes him as a problem of central importance. Instead, he seems to pay great attention to estimates—mostly by early British colonial officials—of population and fief boundaries. The reliability and validity of such demographic data create a problem well-beyond the capabilities of the usual African historian. So much political advantage rests, and always has, with the distortion of data, and the effective use of such data requires special training, which Low clearly does not have.

Where the historian can contribute in describing and explaining particular events, the book also falls short. For example, Rabeh is said to have given up "his apparent intention" of invading the eastern Fulani emirates (p. 174). A close reading of Low's sources does not sustain the argument that Rabeh wished to continue his conquests eastward, nor that he gave up such ambitions. Low neither documents nor explains such assertions. And Buba Yero (Gombe) is said to have withdrawn from operations east of the Hawal River and "then or afterwards abandoned any claim on Biu . . ." (p. 94). This information is utterly crucial to someone working on the development of the Biu political system, but he would look in vain for some explanation of this event. Local traditions in Biu suggest that Buba Yero's incursions led to coordination and organization of intersettlement. This same Gombe leader is said to have conquered the Kanakuru area of Shellen and had it "supervised in some vague manner" (p. 152). Low's writing is frustrating to scholars working on just such problems.

Nevertheless, the book is a start. Scholars now need more detailed political histories of each of these areas. Low has stretched the canvas; others must now paint the picture.

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HELMUTH HEISLER. *Urbanisation and the Government of Migration: The Inter-relation of Urban and Rural Life in Zambia*. Foreword by R. MANSELL PROTHERO. With an appendix on some migration histories by M. G. MARWICK. New York: St. Martin's Press. 1974. Pp. xi, 166. \$15.95.

In 1931 fewer than 25,000 Africans in Zambia (formerly Northern Rhodesia) lived in urban areas; by 1969 the number had risen to some 1,100,000. Helmuth Heisler points out that until after World War II this urbanization occurred despite the opposition of the Provincial Administration. The dominant viewpoint, associated with Lord Lugard's philosophy of indirect rule, emphasized the importance of maintaining the traditional cultural patterns of the village and agricultural ways of life. Urbanization, colonial officials feared, would undermine traditional value systems and structures of authority, disrupt rural life and agricultural enterprise, create racial and labor unrest, and deprive Africans of their social and economic security. Moreover, the cost of housing, policing, feeding, educating, and providing for the health and welfare of a large urban population was more than most European administrators, politicians, and capitalists were willing to bear.

Gradually, the needs of employers in the grow-

ing mining industry and commercial agriculture prevailed over those of administrators. Initially employers used a "Recruited Labor System," monetary inducements, and coercive taxation. But this gave way in the 1930s to a "Casual Labor System," under which Africans went to and from the urban-industrial areas largely on their own initiative in response to social and economic motivations. Yet until the end of World War II Europeans were only willing to provide the Africans primitive "labor camps," consisting largely of dormitories for temporary workers. With growing labor unrest and the need for more highly motivated, skilled, and experienced manpower, "Towns for Africans" were built, making possible a stabilized family life for large numbers of Africans. Nevertheless, it was not until political independence in 1963 that Africans could become committed to "urbanism" as a way of life, with less need to look to their rural homelands for social security.

While Heisler aptly describes and analyzes these developments, certain inadequacies are apparent. Because of the emphasis on patterns of migration, the absence of maps is disconcerting. Occasionally, particularly in the chapter "Women for Urbanization," Heisler goes beyond his sources to speculate about the relationship of divorce, companionate marriage, distance, and nationalism to migration from the rural areas. Here and elsewhere, he might have made greater effort, using interviews, to get an African point of view, which could have increased his confidence about the impact of changing incomes on migration incentives (p. 113). As it is, African motivations for urbanization remain unclear. More serious, the author fails to explore certain policy questions he raises near the end of the book: Is there overurbanization in Zambia? Can and should rural life be made more attractive? If not, what are the consequences of a growing divergence of peasant and urban systems? Without answers to these questions, readers are left with the hope that Heisler deals with them in another book.

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ARYE ODED. *Islam in Uganda: Islamization through a Centralized State in Pre-colonial Africa*. (The Shiloah Center for Middle Eastern and African Studies, Tel Aviv University. Studies in Islamic Culture and History. A Halsted Press Book.) New York: John Wiley and Sons. 1974. Pp. x, 381. \$23.50.

In *Islam in Uganda* Ayre Oded traces the introduction of Islam into the kingdom of Buganda. The work is divided into three parts. Part one deals with the coming of Zanzibar Arabs to Bu-



ganda in search of markets for ivory and slaves during the reign of Kabaka Suna (c. 1832–56), who welcomed them. Oded covers in detail the factors that tended to promote and hinder the influence of Islam, which reached its height about 1875 in Kabaka Mwanga's time. Contrary to current opinion, Islam emerges as having made positive contribution to Buganda by introducing such skills as writing and reading and the use of different styles of clothes and other goods.

Part two deals with the first arrival of Europeans, in particular the explorer Henry Morton Stanley who initiated the effort to undermine Islam and promote Christianity. The ambitions of Khedive Ismail of Egypt (1836–79) to annex Buganda through the work of Colonel Charles Gordon and other agents are well treated, as are the reasons for their failure.

Part three covers the years (1877–84) of intense religious rivalry and intrigue between the Moslems and Christian missionaries, resulting essentially from the latter's determination to oust Islam and end the slave trade. Oded portrays vividly the dilemma and confusion Kabaka Mutesa faced when confronted with conflicting religions (Islam, Protestantism, Catholicism). The leaders of these religious groups are carefully analyzed as are intra-Christian conflicts. Mutesa emerges as a shrewd, highly intelligent ruler, capable of using religion as a tool to maintain his power.

The author then summarizes the progress of Islam after Mutesa's death, through the religious wars of the late 1880s, and brings the work to date by treating Islam in the post-independence era, characterized by division, and the current efforts of President Idi Amin Dada to unify the Moslems.

There are seven appendices, a valuable bibliography, and an index. Oded's book is a very useful, well-researched contribution to an understanding of a neglected aspect of Uganda's formative years that still influences the present.

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J.-M. FILLIOT. *La traite des esclaves vers les Mascareignes au XVIII<sup>e</sup> siècle*. (Mémoires ORSTOM, number 72.) Paris: ORSTOM. 1974. Pp. 273. 120 fr.

PHARES M. MUTIBWA. *The Malagasy and the Europeans: Madagascar's Foreign Relations, 1861–1895*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. xvi, 411. \$17.25.

Off the southeast African coast lies the great island of Madagascar, and to the east of it the small islands of the Mascareignes, Mauritius, and Réunion. Indonesian migrants settled Madagascar during the first millennium A.D., and when Euro-

peans entered the western Indian Ocean around the beginning of the sixteenth century they found vigorous, independent, indigenous polities. The isolated Mascareignes were yet unoccupied when the Portuguese reached them during the same century. In the seventeenth century the Dutch used Mauritius as a stopping place on their route to the East, maintaining a limited presence until the beginning of the eighteenth century. Also in the seventeenth century, the French, who then were active without great result in Madagascar, began their settlement of Réunion. In 1715 they occupied Mauritius.

Slaves from Madagascar, East Africa, and elsewhere were brought to Réunion before the close of the seventeenth century, and during the succeeding century a busy slave trade developed, especially after the arrival in 1735 of the dynamic French governor, Mahé de la Bourdonnais. The magnitude of the slave trade to the Mascareignes long has been a question of interest to scholars concerned with both Madagascar and the East African coast. Using archives in Britain, France, Réunion, Mauritius, Madagascar, and South Africa, as well as the abundant published literature relating to contacts between Europeans, Africans, and the Malagasy, Filliot attempts to answer it. The fruits of his labors make available for the restricted area of his interest a result similar to that of Philip Curtin in *The Atlantic Slave Trade* (Madison, 1969): we now possess a base number to serve as a starting point for all future studies of the slave trade in this region. During the period from about 1670 to 1810 160,000 slaves reached the Mascareignes. Filliot breaks down the imports periodically, as follows: c. 1670–1717, 1000; 1715–26, 10,000; 1727–51, 25,000; 1752–66, 14,000; 1767–1810, 110,000. He calculates that 45 percent of the total Mascareignes slave imports came from Madagascar, 40 percent from East Africa, 13 percent from India, and 2 percent from West Africa. Admittedly, the figures must be treated with caution, but Filliot has carefully analyzed his sources and explained his methodology, so that his readers can reasonably assess his judgments. His figures provide a valuable addition to other recent studies of the slave trade of the western Indian Ocean by C. S. Nicholls, Edward Alpers, Fred Cooper, and Abdul Sheriff. Filliot's scholarship is impressive, and his statistics will have to be accepted as the best available estimate for the foreseeable future. In the remainder of his monograph Filliot provides data on the details of the process of the trade in slaves, including region-by-region analyses of the areas from which slaves were drawn. The bulk of the data, however, relates to Madagascar.

In *The Malagasy and the Europeans* Phares Mutibwa studies the people who dominated much of



Madagascar during the nineteenth century, the Merina of the central plateau. Mutibwa is primarily concerned with the relations between the Merina and the European world from the 1860s to the French conquest in 1895. The Merina and their great leader, Rainilaiarivony, had to deal extensively with Britain and France. For these relations Mutibwa presents a useful account drawn from the major British and French archival collections and from the Malagasy National Archives. The volume adds to our knowledge of this period of Malagasy history when the Merina, by adapting European influences and techniques to their needs, strove to remain free of European rule. But with his focus on the Merina, Mutibwa often over-identifies with their diplomacy; for example, he accepts as valid the dubious claims of the Merina to suzerainty over the Sakalava peoples of Western Madagascar. Nonetheless, since English-language studies of Madagascar are unfortunately rare, Mutibwa's volume fills a niche in the historical bibliography of the great African island.

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IAN LINDEN. With JANE LINDEN. *Catholics, Peasants, and Chewa Resistance in Nyasaland, 1889-1939*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xii, 223. \$16.00.

Roman Catholic missions have been too little studied in Africa, most researchers being more comfortable with the often more accessible Protestant experience. Of the few examinations of the Catholic impact, this one succeeds because of Linden's grasp of the relevant theology and, equally, the social context in which the missionaries labored. For Linden this context included the climate of the sending countries, the constraints and opportunities of colonial rule, and the cultural and other demands of the Africans among whom the priests proselytized.

Linden's best, later chapters discuss the educational effort of the priests and nuns and their attempts, over decades, to create an indigenous-led Church through rigorous training in isolated seminaries. His earlier chapters narrate the pre-colonial attempt to establish Catholicism in Nyasaland (now Malawi), the postpartition starting of stations by the White Fathers as a by-product of their work in Northern Rhodesia (now Zambia), and the subsequent movement into southern Nyasaland of the Montfort Fathers. Linden focuses more upon the White than the Montfort Fathers, but there is less than expected about the Catholic role during the Chitembwe rising. In fact, it is not clear what the "resistance" of the title is meant to convey. There is a glimpse of the

battles over territory between the Catholics and the Dutch Reformed and Presbyterian mission groups, and an explanation of why—after Chitembwe—so many administrators encouraged, even assisted, the Catholic advance.

As a microstudy, this book has substantial merit. But it lacks any comparative dimension. The Catholic experience in Malawi is never contrasted to that of Zambia, East Africa, or Zaïre. Nor is Malawi Catholicism compared more than superficially with Malawi Protestantism.

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BRIAN ROBERTS. *The Zulu Kings: A Major Reassessment of Zulu History*. New York: Charles Scribner's Sons. 1975. Pp. xii, 388. \$10.95.

"The nations, Shaka, have condemned you/Yet still today they speak of you/Still today their books discuss you/But we defy them to explain you." Thus wrote B. W. Vilikazi, a Zulu poet, in the concluding lines of a literary masterpiece. Many authors have tried to provide an explanation for the Zulu phenomenon. They include writers as varied in their background and approach as Peter Becker, C. T. Binns, Max Gluckman, Donald R. Morris, J. D. Omer-Cooper, Leonard Thompson, and E. A. Ritter. Brian Roberts, who has already contributed to a variety of South African themes, now provides a reassessment of the empire that Shaka won and Cetshwayo lost. His book has earned praise from sources as different as the *Library Journal* and Chief Buthelezi, Chief Minister of Kwa Zulu. These good opinions are deserved. The author has consulted a wide range of archival sources in Britain and South Africa, as well as a considerable body of secondary literature. He uses his sources with a sense of discrimination. He writes clearly, and he has an exciting story to tell.

To Roberts, the story of Zululand is a Sophoclean tragedy. Shaka—by disrupting the age-old pattern of tribal settlements, destroying his enemies with methodical thoroughness, and laying waste the surrounding country—founded a kingdom unique in the annals of South Africa. His claim to supremacy, as Roberts sees it, owed as much to territorial aggrandizement as to military supremacy. "That land could represent power (rather than acting as grazing for cattle, the traditional symbol for wealth) was a revolutionary concept in tribal politics. It accounted not only for the rise of the Zulu nation, but for its eventual downfall: a downfall which was heralded by the coming of the first white men."

Roberts would have deserved even better of his readers had he dealt at greater length with the purely military side of Zulu history—with those

questions of tactics, strategy, and supply that played such a vital part in a kingdom founded upon war. Also, he is somewhat overconfident in his use of statistics. No historian should make much of dubious estimates of the kind that 2,000,000 people perished as a result of Shaka's wars, or that 3,000,000 died by reason of Leopold's atrocities in the Congo. Nevertheless, this is a good book. Its appearance indicates how far the popular historiography of Southern Africa has advanced since the appearance of a work like Hugh Marshall Hole, *The Passing of the Black Kings*, 40 years ago.

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### ASIA AND THE EAST

FRANK SWETZ. *Mathematics Education in China: Its Growth and Development*. Cambridge, Mass.: MIT Press. 1974. Pp. 364. \$15.00.

The stated purpose of the author is "to assess the trends in mathematical education at the primary- and middle-school levels in China." Chapter 1 gives interesting examples of native opposition to foreign influence from India in the third century, the Jesuits after 1601, and Europe in the nineteenth century, among others. Chapter 2 describes the establishment, in the late nineteenth century, of a modern school system based first on European and Japanese models and then on American. Chapter 3 begins with a description of Mao's educational theories, and the difficulties—lack of teachers, buildings, books, and chalk—of setting up schools in the remote province of Yen-an. In 1937 there were only 120 schools in these border regions, but by 1949 the number had increased to 1,081.

After their assumption of power in 1949, the Communists were greatly influenced by the USSR, particularly in their theories of education. Chinese students sent to Russia between 1949 and 1958 numbered 61,000, about twice as many as came to America in the forty years after 1909. But many difficulties arose from this alien influence, which was felt by Chinese political leaders to promote an "elitism" inconsistent with true Communist theory. The school system became deeply infiltrated by party cadres, who made demoralizing innovations in 1958 and again in 1960 in connection with the Great Leap Forward.

In the next five years, the fluctuations between "redness" and "expertness" finally produced those uprisings and disorders among students, particularly in the University of Peking, that constitute the beginning of the Cultural Revolution. Finally, in March 1968 army units restored some

semblance of discipline in the schools, which were now taught by teams composed of factory and agricultural workers, political cadres, and revolutionary teachers and students. Instruction had deteriorated in quality, but by 1970 the difficulties arising from Chinese literary tradition and from long-continued foreign influence had been eliminated.

The innovations in Chapter 5 can be characterized by the typically Chinese phrase "walking on two legs," or taking advantage of two kinds of learning—study of books and work in fields and factories, supplemented by psychological experimentation, social events with mathematical games and contests, inexpensive booklets for self-instruction and entertainment, handbooks for farmers, and special journals for secondary-school teachers and students.

The final chapter discusses the extent to which such flexibility could be imitated with profit by Third World countries. The closing sentence optimistically predicts that, as a result of growing mathematical sophistication among workers and farmers, "the industrial potential of the People's Republic of China will become most formidable."

The extensive documentation, in a field in which documentation is notoriously difficult, does credit to the author's industry and resourcefulness.

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JOSEPH R. LEVENSON. *Revolution and Cosmopolitanism: The Western Stage and the Chinese Stages*. With a foreword by FREDERIC E. WAKEMAN, JR. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxxi, 64. \$5.00.

When Joseph Levenson died in 1969, he had completed his *Confucian China and Its Modern Fate* and was deep into a new trilogy on "provincialism and cosmopolitanism." Two volumes were partly drafted, and this brilliant posthumous work, three undelivered lectures, was to be the third. Levenson illuminates the dilemma of post-May Fourth intellectuals. Alienated from the "silk-fan attitudes" of Confucian "high culture," they fostered a new cosmopolitanism "to escape being exhibits themselves . . . for foreign delectation." By analyzing prefaces to Chinese translations of Western plays, Levenson reveals the "rootlessness" of their condition. " 'Bourgeois' cosmopolitans" of the 1930s could savor the "advanced art" of Pirandello or Schnitzler, but their pleasure was ambiguous. For emergent Chinese Communism, the internationalism of Schnitzler's translator had no social purpose. "No red flag was likely to rise from this well-manicured hand."

Why, then, in the 1950s did Chinese Communists continue to condone cosmopolitan translations? Because they had developed a *class* cosmopolitanism of their own. The *jen-min* ("people" as class), which in Chinese Communist analysis virtually constituted the nation, were also the *jen-min* of all nations. In translations of non-Communist plays, a foreign "high culture" was made "people's"—an international term appropriate to the Chinese people. Shakespeare's Henry V led a new nation, but the translator's preface makes the paean abstract, extolling "not the people of England but an age of peoples,"—"and gentlemen of China will think themselves accursed upon St. Crispin's Day." Only after 1957 was "empathy" out. Class struggle went forward, while "the bourgeoisie was still abroad at home." In the "provincial" spirit of the Cultural Revolution, "the sophisticates, detached from the *jen-min* by their culture, were detached from the [nation] by their world-wide affinities with fellow cosmopolitans."

Levenson's perception of Chinese intellectuals was sensitized by his own dilemma as a believing Jew. In an elegant foreword, Frederic Wakeman shows how Levenson strove to define Judaic particularism (the history, the people) against the "spurious universalism" of Christian tradition. His outer scholarship expressed his inner faith: in his life as in his work, "a tension which prohibited the merging of opposites was dynamically creative."

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RICHARD J. SMETHURST. *A Social Basis for Prewar Japanese Militarism: The Army and the Rural Community*. (Published under the auspices of the Center for Japanese and Korean Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1974. Pp. xxi, 202. \$14.00.

This book presents a narrowly focused but provocative interpretation of the social dynamics of rural Japan between 1910 and 1945. Richard J. Smethurst argues that in 1910 the Japanese War Ministry created a comprehensive network of Imperial Military Reserve Associations (*Teikoku zaigo gunjinkai*) in order to promote national unity and to foster grass roots support for the army. The War Ministry subsequently encouraged the formation of the Greater Japan Youth Association, youth training centers, and the Greater Japan National Defense Women's Association, all of which were controlled at the local level by the Reserve Associations.

Drawing upon local histories and detailed case

studies of several Japanese villages, Smethurst demonstrates how the Reserve Associations, in particular, merged into rural communities. Unlike the national school or police systems, the Reservists had branches in every tiny hamlet, the basic unit of rural society. Reservists served in important capacities within these communities, as police or firefighters, for example. Usually all eligible males joined the local branch, and its leadership often reflected the local economic power structure. Through the Reserve Associations, the army could indoctrinate the Japanese peasantry with patriotic and military values. The peasants became "national villagers" who supported the army, as loyalty to the hamlet became synonymous with loyalty to the nation.

Smethurst provides important evidence that there was no sudden eruption of militarism in Japan, since he shows how the army expanded its influence gradually after 1910. Yet this book offers a limited picture of prewar rural Japan, as there is no analysis of serious rural economic problems or of the relationship between the Reserve Associations and local leaders who were agents of the political parties. Also, one must question whether the Reservists' encouragement of patriotism and obedience alone led to xenophobia in Japan.

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P. HARDY. *The Muslims of British India*. (Cambridge South Asian Studies, number 13.) New York: Cambridge University Press. 1972. Pp. ix, 306. Cloth \$15.50, paper \$5.95.

FRANCIS ROBINSON. *Separatism among Indian Muslims: The Politics of the United Provinces' Muslims, 1860-1923*. (Cambridge South Asian Studies, number 16.) New York: Cambridge University Press. 1975. Pp. xiii, 468. \$22.50.

ALBERT CHRISTIAAN NIEMEIJER. *The Khilafat Movement in India, 1919-1924*. The Hague: N.V. de Nederlandsche Boek- en Steendrukkerij V/H H. L. Smits. n.d. Pp. vii, 263.

ALEXANDER F. BAILLIE. *Kurrachee: Past, Present and Future*. (Oxford in Asia Historical Reprints from Pakistan.) New York: Oxford University Press. 1975. Pp. xx, 269. \$20.75.

The books under review deal with the Muslims of India during British rule. Hardy's study is a general, readable discussion of the varying impact of British policies upon India's Muslims. Robinson's monograph is a revision of his doctoral dissertation, and as such, is much more restricted in scope, concentrating on the Muslims in one province during the era of emerging Muslim self-consciousness as a separate political group. Niemeijer's work is

also a dissertation dealing with Indian Muslims during a particularly significant political movement. The reprint of Baillie's *Kurrachee* [sic] will be commented upon later.

There are a number of points of comparison among the first three works. Each deals with British policies toward Indian Muslims and the Muslims' responses to those policies. Each looks at the evolution of the Indo-Muslim community, especially in the political context. Hardy and Robinson, in discussing British policies toward Muslims, show that there were differences according to regime and region. In this welter of contradictory policies and priorities, the myth of a carefully worked out policy of "divide and rule" is laid to rest. Hardy characterizes British policy as one of "balance and rule," an attempt to balance a number of interests, regional and professional as well as religious. Thus, the evolution of Muslim separatist politics was less the result of Muslim response to the categories perceived by their British rulers, than the unpredictable combination of British attempts to balance interests with the Muslims' evolving sense of identity. This identity was based on the legacy of political supremacy and earlier religious reform movements, in addition to the political and economic pressures of British rule.

Robinson, however, gives to successive British governmental reforms the predominant role in the emergence of Indian politics and, consequently, of Muslim separatism. Political rivalries emerged because of competition for new patronage, cast in the molds of British categories. In spite of occasional lip service to "firm religious beliefs," Robinson views religion and ideology as irrelevant in the creation of Muslim separatism; the competition for bureaucratic favor and electoral advantage was all-important. His tone is indicative of a point of view straight out of the British records. He sees the use of religious ideas and symbolism in the political context as rank opportunism, designed to hoodwink the credulous masses and to increase the circulation of the radical young Muslim press. This tone is not conducive to understanding the Muslim politicians in their own terms. Granting the importance of British policies in the political development of India, one nevertheless must take into account a variety of cultural intangibles and accept their legitimacy in the political process. Hardy seems to be attuned to this need; Robinson tends to be insensitive to all but the "hard" political and economic factors.

If the British Raj was not monolithic in its dealings with Indian Muslims, neither was the Muslim community monolithic in its response. Here again, Hardy and Robinson make useful contributions in cutting the myth of Muslim unity into its com-

ponent parts. Hardy clearly shows the different regional and class distinctions among Muslims and their varying responses to the challenges of Western education and new employment patterns. Robinson analyzes the factional divisions within Muslim leadership of the United Provinces, dubbing the factions the "Old Party" (progovernment) and the "Young Party" (pronationalist). Here, too, the differences between the two parties were based as much on differences of style and personality, and on religious and cultural variables, as on attitudes toward working with government.

Niemeijer's work, both in dealing with British policy and with the Muslim response to it, is considerably more monolithic in approach. The author occasionally shows that British bureaucrats disagreed over policy, but he seems to lose sight of this fact in his generally simplistic analysis. He spends a great deal of time trying to define "nationalism" and "communalism" and to use these definitions in his narrative. This approach leads him to ignore the conflicts among Muslim politicians, a result which is particularly disastrous for a study of the Khilafat movement. His assumptions lead him to treat the Indian Muslims as an undifferentiated group, whereas the Khilafat movement sought to create a political consensus among a number of groups and factions. Niemeijer certainly does not help increase our understanding of religio-political movements, and he betrays an insensitivity to the importance of ideological factors.

Perhaps the reason for inadequate understanding of the complexities of Muslim separatism lies in the sources each author uses. Hardy is best able to understand his subjects because of his background in Islamic scholarship and his use of Persian and Urdu sources in addition to British records. He thus provides scholars with a useful, insightful synthesis of much recent research. Robinson has done a painstaking job of research in British records, nationalist newspapers, and private papers. He has also used some Urdu sources in translation. It is unfortunate that this potentially valuable piece of research is marred by a bias which severely limits its usefulness. Niemeijer's sources are limited to British records and secondary works in Western languages, and his point of view is myopic. His work is a rehash of much that has gone before, with no new insights.

Baillie's *Kurrachee*, originally published in 1890, is a chatty, rambling, and thoroughly delightful collection of miscellany about the sleepy port he felt certain was destined for greater things. The work contains religious and ethnographic observations, as well as much that will be of interest to urban and economic historians, especially those inter-



ested in the development of ports and railways under the Raj.

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ARUN COOMER BOSE. *Indian Revolutionaries Abroad, 1905-1922: In the Background of International Developments*. Patna: Bharati Bhawan. 1971. Pp. xv, 268. Rs. 30.

EMILY C. BROWN. *Har Dayal: Hindu Revolutionary and Rationalist*. Tucson: University of Arizona Press. 1975. Pp. xiv, 321. \$7.95.

Indian revolutionaries played important roles in international politics during the last four decades of British rule in South Asia. Although their barrage of inciting literature and occasional violent acts did not bring the imperial government to its knees, the revolutionary menace did force compromise by the British and served as a reminder of alternative avenues of political expression if concessions were not made to Indian nationalist demands. In addition, militants in Europe and North America affected diplomatic relations, creating tension between England and her nominal allies. In Canada and America the revolutionaries also became involved in local politics and built alliances with respectable individuals and parties.

Both these studies explore the many dimensions of Indian revolutionaries abroad. Arun C. Bose connects activities in Asia, Germany, and North America between 1907 and 1919. Balancing archival material with printed sources, he traces how bands of Indian radicals in different locations formed links during the First World War and ultimately proved ineffective in sabotaging British administration in India. Although Bose's writing is dry, and his conclusions at times are influenced more by how revolutionaries and their sympathizers or opponents tended to view the movement rather than by facts, the assessment probably will remain the best of the half-dozen books surveying the subject.

Emily Brown, however, goes far beyond writing just another synthesis or recounting deeds and misdeeds among revolutionaries. Focussing on the life of one individual, Har Dayal, she sheds new light on what Indians were doing outside the subcontinent while providing a lively and highly readable biography of an enigmatic Indian intellectual. The product sets a new standard for careful tracing of stray documents and arranging them in meaningful fashion.

In 1905 Har Dayal came to England as a bright young scholar with a grant and much academic

promise. Within two years he changed first into a bitter critic of imperial rule, and then into a hardened propagandist and plotter. But as Brown clearly demonstrates, Har Dayal's antipathy toward colonial rule was not solely a matter of politics; it rested ultimately on a concern with the nature of Indian civilization and tradition. At each step of his career, Har Dayal mixed scholarly research and philosophical speculation with attacks against the immediate cause of national degradation, foreign rule. His tendency to move from project to project, friend to friend, and country to country, reflected psychological uncertainty and unrest growing from a preoccupation with evolving a satisfactory philosophical statement while concomitantly playing a meaningful role in everyday affairs. Har Dayal's public life thus can be broken into two major segments: 1907-19, a period of revolutionary zeal and activism; and 1919-39, a time of enforced exile when he attempted to deal with India's many problems on an intellectual plane. Thwarted from returning home by British fear, he wrote books on philosophy and Hinduism, worked toward a synthesis of Western and Indian ideas, and gradually became more conservative in politics.

Har Dayal was not a typical Indian revolutionary, but by carefully studying his life and the milieu in which he lived, Brown has broadened our understanding of organizations and individuals involved in the futile effort to overthrow the British. She and the University of Arizona Press are to be congratulated for producing a major contribution to modern Indian intellectual and political history.

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Columbia

R. J. MOORE. *The Crisis of Indian Unity, 1917-1940*. New York: Oxford University Press. 1974. Pp. xii, 334. \$9.00.

In a remarkable speech in the House of Commons in 1858, John Bright argued that the united India created by the British served only their own greed and vanity, with despotism a necessity to preserve a political structure that denied the complexity of the subcontinent's historical experience. For the sake of the people of India, he urged that the area be divided into at least five separate states. R. J. Moore does not mention Bright's prescient argument, but his book is, in effect, a comment on the ironies and anomalies that became apparent as representative government and independence became political realities. When the process of devolution of power got under way with the Montagu-Chelmsford reforms at the end of the First World



War, the form of nationalism articulated by the Indian National Congress had already made it an article of faith that the imperial territorial legacy would be handed down intact to a liberal, representative state. The British, despite the conventional arguments of national historians, shared the passion for maintaining political unity, if not for democracy.

Moore traces in considerable detail the development of the strains in Indian political life from the announcement in 1917 of the British intention to move toward responsible government to the point in 1940 when the strains became deep fissures. The central chapter deals with the crucial period in 1930 when, under Gandhi's leadership, the Congress evolved new strategies to hold together a badly divided Congress. According to Moore, Gandhi concluded that creating these strategies was more vital to Indian unity and freedom than a continual and frustrating search for a constitutional formula to satisfy Muslims, Hindu liberals, British Conservatives, and Indian Princes. His decision was to mount a civil disobedience campaign to expose British rule in India as a despotism manipulating the Indian economy for its own selfish ends. He formulated eleven demands that made no reference at all to constitutional issues, but only to subtle linkages of morality and economics, such as prohibition, the salt tax, customs duties, and the exchange rate. He was thus able to secure the backing of the mercantile community for "an experiment in anarchy," with the Salt March its great symbol. Thus, unity within the Congress was purchased by ignoring the growing estrangement with the Muslims.

The India Act of 1935 was intended to take India toward semiautonomous provinces, but the Congress forestalled the Act's intentions by controlling its provincial governments through a unitary party structure. According to Moore, the program of gradual devolution, the strategy the British hoped would maintain unity, led "inexorably towards division," since the Muslim leadership was excluded from power.

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NICHOLAS MANSERGH and PENDERL MOON, editors. *The Transfer of Power, 1942-7. Volume 5, The Simla Conference: Background and Proceedings, 1 September 1944-28 July 1945.* (Constitutional Relations between Britain and India). London: H.M. Stationary Office; distrib. by Pendragon House, Palo Alto, Calif. 1974. Pp. xcii, 1346. £17.50.

Documents on the Simla Conference, found in the latest volume of this series (Vol. 5), show a marked

rise of political tensions. As an observer of a classic drama, one can sense the rising pitch of emotions and the tightening strain of nerves as events moved to their ultimate, tragic climax—Partition. Once again, this volume shows that each party displayed semblances of reasonableness and pacificity. A slow thaw came upon political relations in India, due chiefly, as is often and explicitly shown, to the prospect and reality of victory in the European war. With their homeland no longer threatened, British rulers could afford magnanimity. They did not withdraw the Cripps pledges of further constitutional advances, despite the Quit India Rebellion and its aftermath. In order to redeem the pledges, however, the British saw further Anglo-Indian dialogue as a necessary preliminary. That such a resumption of talks should take place earlier rather than later was of great concern to the viceroy. He felt that while at the moment the British *raj* was "sitting pretty," its long-term prospects were far from comfortable. Unless imaginative steps were taken to assure political advances by agreement among leaders of the major parties in India, the probability of serious unrest and violence would grow. Leaders in Britain were not so quickly persuaded; members of the War Cabinet and especially Churchill questioned the wisdom of such a move and the forms it should take. The debates they engendered, taken from Indian and British records, provide unusual glimpses into the workings of the "official mind" in Britain as it moved toward its penultimate decision on "the Indian problem."

At the conclusion of debate the British Cabinet agreed to call a conference of Indian political leaders, to be held at Simla on June 25, 1945. Documents in the volume relate the story of the conference in graphic detail. The difficulties of the viceroy and his advisers become glaringly apparent. The imprisoned Congress Working Committee, whose participation was so important to the conference's success, had to be released. Gandhi's role with the conference had to be defined—first, because he claimed that he was not a member of the Congress; and second, because his power over the Congress was so obvious. Deliberations followed an erratic course. Ultimately, they broke down and the conference failed. In the view of the viceroy, responsibility for the breakdown lay in "the real distrust of the Muslims, other than National Muslims, for the Congress and the Hindus."

Of course, the crucial events of the conference cannot be seen apart from other important developments—relations with other countries, plans for industrial development in India, India's representation at the San Francisco Conference, supplies of food, and questions of prospective commercial re-

lations between an independent India and Britain. Taken as a whole, these documents provide a fascinating insight into the workings of one of the great political systems of modern times.

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University of Wisconsin,  
Madison

## UNITED STATES

WILCOMB E. WASHBURN. *The Indian in America*. (The New American Nation Series.) New York: Harper and Row, 1975. Pp. xix, 296. \$10.00.

This volume in the New American Nation Series must have been one of the most difficult assignments in the series. The militancy in recent years of Indians and their supporters has made it increasingly difficult to deal objectively with any aspect of the history of Indian-white relations. Wilcomb E. Washburn has retained his poise remarkably well, and there is a studied effort to see both sides of any issue. He did not shrink, however, from taking a position on the American Indian Movement, which will distress its adherents.

Overall, this is easily the best survey of Indian-white relations. Washburn was an early advocate of ethnohistory (the effort to bridge the disciplines of history and anthropology), and it is reflected in this volume. The author draws upon the most recent scholarship in history and is equally at ease in the literature of anthropology. The result is a depth and breadth to the discussions which make it invaluable as a reference work. Moreover, his clear, direct style makes for easy reading.

No survey of such a complex and controversial topic can be completely satisfactory to any reader. Some may conclude that the balance of the book gives the colonial and early national periods undue space and compare the treatment of the Pequot and King Phillip wars with the failure even to mention Custer's battle on the Little Big Horn. This reviewer differed with several specific judgments made by Washburn, for example the defense of treaties as "more often than not, the bargain struck by two autonomous forces who weighed carefully the advantages accruing to each from the agreement." Nor could this reviewer agree with the significance assigned the period 1845-55. If the western Indians were "humbled and reduced," it certainly was in the twenty years following 1855, not in the years 1845-55.

These were exceptions, however, to the generally excellent quality of the volume. As one closely involved in the preparation of the new multivolume edition of the *Handbook of North American In-*

*dians*, Washburn was the best-posted historian to undertake this task, and he has not disappointed us.

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Fredonia

LEWIS P. SIMPSON. *The Dispossessed Garden: Pastoral and History in Southern Literature*. (Mercer University Lamar Memorial Lectures, number 16.) Athens: University of Georgia Press, 1975. Pp. x, 109. \$6.00.

In *The Dispossessed Garden*, Lewis P. Simpson finds a unifying theme in the literature of the South from the seventeenth century to the present: the attempt by Southern writers to accommodate their unique history with the pastoral myth of the New World as a redemptive garden.

Arguing that Virginia was as much a covenant community as New England, Simpson asserts that Virginians from William Byrd II through Thomas Jefferson found an important place for black chattels in this society. Slaves were unobtrusive gardeners, a necessary part of a "world which the Southern imagination would unsuccessfully endeavor to represent in terms of a pastoral garden of the chattel, seeking to make a covenant between the literary mind and chattel slavery, to make slavery the condition of the independent mind" (p. 32-33).

Southern writers of the antebellum period attempted to reconcile a conflict between the institution of slavery and the pastoral image "by giving the chattel a pastoral relation to the land" (p. 48) and by producing "a literature of radical alienation from modernity" (p. 52). Writers of the early twentieth-century Southern Renaissance created a literature of alienation based upon a "covenant with memory and history" (p. 99). In the post-Faulkner South, "a covenant with the existential self" (p. 99) dispossesses the culture of alienation and offers new challenges to the survival of the literary imagination.

The author has chosen a wide selection of impressive illustrations from the literature and history of the South to support his thesis. Historians with an appreciation for the possibility of using myth and literary symbolism to understand history will find much in this small volume worthy of reflection and debate.

E. STANLY GODBOLD, JR.  
Valdosta State College

PETER J. COLEMAN. *Debtors and Creditors in America: Insolvency, Imprisonment for Debt, and Bankruptcy, 1607-1900*. Madison: State Historical Society of Wisconsin, 1974. Pp. xiii, 303. \$17.50.

This is a frustrating book. It deals with subject matter of vital concern to legal and economic historians; the author spent seven years examining virtually every relevant printed source and a small but revealing sample of manuscript court records and debtors' petitions. Yet at the outset Coleman warns us that his study "does not pretend to be definitive. Rather it is a preliminary exploration" (p. ix). Unfortunately, the admonition is well taken. The book makes substantial contributions on a variety of topics. But the author's very plan for the volume foreclosed any possibility of systematically testing hypotheses regarding the impact of economic change on the public law of debtor-creditor relations.

The structure of the book is deceptively straightforward. Part One consists of three introductory chapters which survey debtor-creditor law in the colonial era, congressional experimentation with short-term bankruptcy laws during the nineteenth century, and judicial construction of the contract and bankruptcy clauses of the Constitution vis-à-vis state action in the field. After 1824, when a bitterly divided Marshall Court handed down *Ogden v. Saunders*, it was clear that state governments had ample authority to abolish debtors' prisons and, so long as Congress failed to supersede state laws, to enact prospective bankruptcy programs as well.

Parts Two through Four contain, in effect, fourteen individual monographs—one for each of the original thirteen states plus Vermont. Here Coleman digests each jurisdiction's applicable statutes, deftly supplements findings drawn from legal records with secondary materials, and painstakingly describes each state's idiosyncratic chronological and institutional development in the debtor-creditor field. These narrative chapters form the heart of the book and will be of considerable interest to state and local historians. But legal and economic historians are apt to be disappointed. We are left wondering, for example, why Massachusetts enacted a bankruptcy law as early as 1837, yet only three other states followed her lead before 1894. Rarely does Coleman adequately interpret what he reports; hence the comparative dimensions of his subject remain virtually unexploited.

Part Five contrasts sharply with the preceding sections both in substance and tone. In two chapters of "perspectives," one each on the debtors' prisons and bankruptcy, Coleman suddenly plunges into business history and offers some exciting observations on the interaction between debtor-creditor law and changes in the very style of lending. "In the long run," he concludes, "the debtors' prison disappeared because it was obsolete" (p. 268). Beginning in the mid-nineteenth century lenders developed more reliable in-

stitutional mechanisms for securing loans; creditors discovered that the power to imprison was superfluous in a marketplace served by monetized commercial paper, crop liens, pawnshops, garnishment, and conditional sales contracts. Bankruptcy relief was a more difficult matter. Policy-makers had to balance the demand for laws to encourage risk-bearing entrepreneurship with the nation's lack of fluid capital and the consequent need to maintain Americans' credit rating in national and international money markets. Moreover, advocates of bankruptcy laws had to overcome their fellow citizens' ingrained belief that discharging debtors was immoral. For many nineteenth-century Americans it was one thing to abolish debtors' prisons or to enact temporary stay laws in times of economic distress, and quite another to accord insolvents the right to discharge their obligations altogether. Finally, Coleman suggests that the slow acceptance of the discharge concept stemmed, in part, from the legislative process itself. Except in New England, the state legislatures simply lacked the independent energy to forge a coherent bankruptcy policy "which would benefit lenders, borrowers, and society, and which at the same time would be simple to administer, free from fraud, and relatively stable" (p. 277). Thus drift, default, and fleeting experiments—familiar themes to nineteenth-century legal historians—ensued until Congress finally enacted a national measure in 1898.

Coleman's concluding "perspectives" are important but out of place—they are not linked to data derived from the state-by-state narratives; nor do they account for state and regional variations in the chronological evolution of debtor-creditor law. In short, they are hypotheses which ought to have been tested in the body of the text. Nevertheless, Coleman's keen appreciation for detail and his penetrating observations make this a valuable book. A more complete "preliminary exploration" is difficult to imagine.

CHARLES W. MCCURDY  
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CARL BRIDENBAUGH. *Fat Mutton and Liberty of Conscience: Society in Rhode Island, 1636-1690*. Providence: Brown University Press. 1974. Pp. xxiv, 157. \$8.00.

It is a pleasure to watch Carl Bridenbaugh at work as he coaxes a fresh and revealing history of early Rhode Island out of a wide range of manuscript and published sources. He reminds us that the Narragansett Bay country developed as a region of commercial farming—often on a scale that was large by the standards of the day—and that there was much more than religion to the colony's story.

Bridenbaugh's larger purpose is to show from the Rhode Island example that the "central theme of early American history is to be found in the nature and achievements (or failures) of an agricultural-commercial society" (p. xxii). In urging the guild back to the farm, he recognizes the paradox. How can a historian who has spent a lifetime studying various aspects of colonial urban life now repudiate or at least minimize the importance of his own career?

The answer, of course, is that he only appears to be discounting his own work. Bridenbaugh, however, misses another paradox; in a more junior historian, his strident call for a return to the true essentials and his indictment of what has been fashionable among colonial historians in recent years would be dismissed as youthful carping. "American historians are all city slickers," he proclaims; the "neophytes of the historical profession . . . have not the slightest insight into . . . how much different the seventeenth century was from the twentieth" (p. xix). Even were these allegations true, which they are not, the tone is unfortunate, since it diverts attention from the substantial contribution made in this little book.

With infinite patience and care, Bridenbaugh reconstructs the process of farm making; produces fascinating evidence on the deliberate, systematic, and large-scale investment in commercial farms; properly assesses the importance of climate, topography, and the many islands to the development of both pastoral and cropping activities; and links commercial agriculture to the colony's maritime trade. It is certainly to be hoped that other historians will pay him the supreme compliment of studying in similar detail the agriculture of the rest of New England, though one would also hope to see a higher level of synthesis and greater skepticism about lambing rates.

PETER J. COLEMAN  
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Chicago Circle*

ARTHUR J. RAY. *Indians in the Fur Trade: Their Role as Trappers, Hunters, and Middlemen in the Lands Southwest of Hudson Bay, 1660-1870*. Buffalo, N.Y.: University of Toronto Press. 1974. Pp. xii, 249. \$4.50.

Although limited to the Crees and Assiniboinés of present Manitoba and Saskatchewan, this is a model study of the fur trade in all its economic aspects. It consists of a series of topical essays, each thoroughly done. Arranged chronologically and buttressed by graphic figures and statistical tables, they form a very satisfactory history. Illuminating inferences from scattered statements in primary sources demonstrate the author's mastery of his subject.

Some of his interesting observations are that brandy gave the French traders an advantage that their English rivals were only partially able to offset with Brazilian tobacco; that droughts, fires, and epidemics were nearly as important as fierce competition and over-trapping in causing the decline of the fur-bearing animals; that sundries were prized by the Indians as much for their trade value to other Indians as for their intrinsic utility. Demographic changes are well delineated, but social changes are less often discussed. The viewpoint of the author is always that of the economist, never that of the moralist or the dramatist.

The overall conclusion is that the fur trade enabled the Indians to maintain a partnership with the whites for over two centuries. Never an equal partnership, it enabled them to survive and to retain a measure of independence. The Indians adjusted to changing conditions over which they had no control, much as this reviewer does in accepting his first paperback in a lifetime of reviewing.

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ERNEST BENSON LOWRIE. *The Shape of the Puritan Mind: The Thought of Samuel Willard*. New Haven: Yale University Press. 1974. Pp. xi, 253. \$12.50.

Samuel Willard's seventeenth-century treatise on Puritan theology, *A Compleat Body of Divinity*, which has long been considered lengthy, difficult, dry, and tedious, is admirably explicated by Ernest Benson Lowrie. He arranges, clarifies, and analyzes the major doctrines in this classic compendium in a thorough manner. Unfortunately, he professes to do no more than give a "careful reading" of his text, and he specifically disclaims any effort "to trace the various ideas to their origins." This is regrettable, since the author affirms in the preface that Willard's "thought exhibits the depth and breadth of the cultural heritage New England received from England and the medieval universities." In his text, moreover, he draws a parallel with the "medieval mind," and he devotes a large part of his bibliography to Willard's European heritage. In a short epilogue, Lowrie suggests that the religious ideas of both Benjamin Franklin and Jonathan Edwards grew out of Willard's theological system. He offers no specific parallels, signs of continuity, or formal relations, however, except the biographical coincidence that Willard officiated at Franklin's baptism. Neither the presumed medieval antecedents nor the suggested eighteenth-century connections are developed in the main body of the book. It cannot, therefore, be considered in any sense a study in the history of ideas, for there is no history presented. Willard's



notions are treated in an absolute vacuum. George Keith, for example, is introduced as one of Willard's intellectual opponents, but nothing is said to identify him, not even that he was a Quaker. A list of Willard's publications reveals that all were printed in Boston, with the exception of some observations on witchcraft which were brought out by Bradford in Philadelphia. An intellectual historian might wonder why.

One major hiatus exists in Lowrie's exposition, even granting the legitimacy of studying a theological system abstracted from its predecessors, contemporaries, and followers. This hiatus is the debate over freedom and necessity. Both Calvin and Edwards denied the freedom of the will, as did many of Willard's contemporary Puritans. Lowrie does not raise the issue at all in the sense of a controverted point, but treats Willard throughout as taking the doctrine of freedom for granted. The latter's ingenious distinction between absolute and natural necessity would have offered an opportunity to determine whether, in Willard's view, pure contingency is ever possible. Unfortunately, Lowrie does not pursue inquiries of this nature.

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ROBERT E. LEE. *Blackbeard the Pirate: A Reappraisal of His Life and Times*. Winston-Salem, N.C.: John F. Blair. 1974. Pp. viii, 264. \$8.95.

Though Blackbeard is almost legendary, surprisingly little is known about him except during 1717-18 when he attracted notoriety as a pirate. Such basic facts as his birthplace and his real name are obscure at best. After the peace in 1713 he apparently refused to remain an inconsequential legitimate mariner, continued sea-roving, and evolved from a privateer into a pirate.

Lee has mined Virginia and North Carolina sources, perhaps too much so, because sometimes in the narrative Blackbeard gets lost in the minutiae. A more fundamental weakness is that Lee does not fully comprehend the nature of English piracy in the Western Hemisphere. Piracy flourished in the 1660s and 1670s when Henry Morgan and his fellow buccaneers brought their booty from the sack of Panama to Jamaica. But during the 1670s European mother countries began to curb their American freebooters. Morgan became Jamaica's lieutenant governor, and before 1700 much of Port Royal had sunk beneath the waves. Pirates of a later generation, such as Blackbeard, were driven to the Bahamas, the Carolinas, and other peripheral areas. Lee's attempts to make Blackbeard another Morgan, or even a Sir Francis Drake, do not succeed. Moving beyond the Outer

Banks the author sometimes founders, such as in placing the Bahamas in the Caribbean, asserting that the Navigation Acts were a prime cause of piracy, or even putting tri-colors on French vessels of the period.

Lee portrays Blackbeard's North Carolina far more favorably than did Virginia's William Byrd. According to Lee, Governor Spotswood, who illegally captured Blackbeard, was almost the villain, while Governor Eden of North Carolina was a paragon of virtue. This argument has at least some merit, as do various aspects of local history. Historians are not likely to agree, however, with many of Lee's dicta. Americans today undoubtedly would still speak English rather than Spanish even if Blackbeard and his associates had never existed.

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FRANCIS F. GUEST, O.F.M. *Fermín Francisco de Lasuén (1736-1803): A Biography*. (Publications of the Academy of American Franciscan History. Monograph Series, volume 9.) Washington: the Academy. 1973. Pp. xx, 374. \$17.50.

George Vancouver in 1793 attempted to immortalize the second great head of the Franciscan missions in California, Fermín Francisco de Lasuén, by naming the points of San Pedro Bay after him. Point Fermín to the west stands out boldly. That name is currently in use, but almost no one asks who Fermín was. Point Lasuén to the south, though a source of contention during the tidelands litigation, has an uncertain location and is almost never mentioned. Lasuén's contemporaries praised him highly, notably Governor Borica, Lapérouse, Malaspina, and Vancouver, and historians, from H. H. Bancroft on, acclaim him even in comparison to his famous predecessor, Junípero Serra. Lasuén administered Serra's nine missions plus nine more and over a longer period. In his time the missions were rebuilt more sturdily and with tile roofs. They grew prosperous, and the number of neophytes increased. But the word count on Lasuén will never catch up with that on Serra. A biography of the first father-president appeared in 1787, and several others followed; the present work 186 years later is the first of Lasuén.

Francis F. Guest, one of a cadre of modern Franciscans concentrating on this historical microcosm, benefits from earlier scholarship such as the documentary studies by Herbert Bolton, Maynard J. Geiger, Antonine Tibesar, and Theodore E. Treutlein on topics prior to Lasuén's presidency. Guest also makes good use of Finbar Kenneally's two volumes of Lasuén's writings. The author's research extends further and involves Lasuén's youth in the Basque provinces and his work at San



Borja in Baja California, at San Diego, San Juan Capistrano, and elsewhere during the seventies and early eighties. There are chapters on his relations with military governors Rivera, Fages, and Neve, with whom Serra clashed. Guest deals with the issues of how to avoid single missionary assignments, what to do about runaway Indians, and the duty as presidial chaplains. He is frank about missionaries unfit for assignment to the frontier and is modest in his estimate of achievements in arts and crafts. His stout volume, rich in insights on California in its middle years as a Spanish outpost, is the biography that Fermín Francisco de Lasuén deserves.

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BERNHARD KNOLLENBERG. *Growth of the American Revolution, 1766-1775*. New York: The Free Press. 1975. Pp. xxii, 551. \$15.00.

ALAN ROGERS. *Empire and Liberty: American Resistance to British Authority, 1755-1763*. Berkeley and Los Angeles: University of California Press. 1974. Pp. xi, 205. \$8.95.

Published fifteen years ago, the late Bernhard Knollenberg's *Origin of the American Revolution: 1759-1766* is certainly one of the half-dozen most important books to appear on that subject. Based on prodigious research, it established that the shift toward a more restrictive colonial policy by Britain occurred not in 1763 but at least as early as 1759 and demonstrated in detail how a variety of British measures seriously antagonized many colonists between 1759 and 1764 and thereby constituted an important precondition for the violent outburst against the Stamp Act in 1765-66.

These two volumes extend Knollenberg's earlier discussion. Examining the experiences of the northern colonies—primarily Pennsylvania, New York, and Massachusetts—during the Seven Years' War, Alan Rogers argues that the "child, America, was already rebellious and plagued by doubts about the power held by her parent, Great Britain," by 1759. "While the struggle to drive France from the North American continent was being waged," he contends, "Americans from every social class experienced firsthand, or had some cause to fear the use of arbitrary [imperial] power." Some American discontent derived from the royal attempts to centralize Indian administration and still more from the overt condescension of British regulars toward American provincials and the "discriminatory military structure" that assigned American officers and soldiers to subordinate roles. Most, however, arose from the insistence by British military commanders that military

necessity overrode all other considerations. "Granted sweeping powers by the Crown," they "imposed embargoes on shipping, ordered press gangs into the streets and countryside to seize men and property, forced citizens to quarter soldiers in their homes, and insisted that the authority of colonial political agencies was subordinate to their own military power." Whereas in 1754 Americans had thought that their "liberty could be maintained while imperial authority was increased," their wartime experiences "stimulated the development of attitudes that led them to take an anti-imperialist stance and gave rise to the question of whether empire and liberty were [any longer] compatible."

Largely because of the narrowness of the research base and a limited focus, this reading of the causal importance of the Seven Years' War seems to require several important modifications. First, the author's failure to relate wartime military policy to larger currents of change in imperial behavior toward the colonies prevents him from seeing that the "assertive imperialism" began not in 1755 but in the late 1740s and was in many areas actually relaxed during the early war years when colonial assistance against the French seemed necessary. Second, the colonial reception of the Albany Plan of Union indicates that even before the war American leaders had strong feelings "about the dangers of arbitrary [British] power." Third, given the extraordinary outpouring of British patriotism by many American leaders during the war, it is by no means clear that Americans were so alienated by their wartime experiences as the author suggests. Finally, it is probable that the importance of the Seven Years' War for the origins of the Revolution lies less in the extent to which it may have contributed to American unhappiness with British rule than in still other psychological consequences and structural changes it produced: Americans had expectations for better treatment and a larger role within the Empire at the same time that there was less need for Britain to be conciliatory toward the colonies. Given these qualifications, all that can be said with confidence is that the particular wartime experiences Rogers describes confirmed the suspicions of many Americans about Britain's arbitrary exercise of power. Such confirmation probably intensified residual anxieties over the constitutional safeguards of colonial liberty and property, but was probably insufficient to weaken significantly the strong ties of affection to the British empire.

The Knollenberg volume does not suffer from limitations of evidence or perspective. Like the *Origin*, to which this book is a sequel, it is built upon extensive research. Indeed, the scholarly apparatus is actually intrusive. The text accounts for

only about thirty-five percent of the total number of pages, while the notes and bibliography take up almost fifty percent and twenty-seven appendices most of the remaining fifteen percent. Focusing primarily on the main developments in the dispute between Britain and the colonies from the repeal of the Stamp Act of 1766 to the outbreak of war at Lexington and Concord in 1775, it is useful for the authoritative and concise summaries it provides of the central issues and developments in the dispute and for reminding us of the significance of the substantive political issues which activated those determinants.

Yet it does not match its predecessor in scholarly importance. As stated in the front matter, Knollenberg's thesis is that "the Provocative Conduct of the British Parliament and Government" between 1759 and 1775 wrought such a profound "Change in the Minds and Hearts" of the colonists that neither "appeals to self-interest" nor "ties to a common heritage" could "prevail against the force of passion aroused by the shedding of American blood" at Lexington and Concord. Unfortunately, this thesis rarely informs the author's subsequent handling of his material, so that the volume lacks that clarity of argument and marshalling of supporting evidence that have been such a refreshing feature of Knollenberg's earlier works. At the same time, although the volume does provide the fullest discussion yet available of some previously little-noted irritants in imperial-colonial relations, it does not often leave the central areas of dispute to explore those on the periphery. Thus we are offered no discussion of the idiosyncratic, but important, ingredients that contributed to the make-up of a revolutionary mentality in colonies other than Massachusetts and New York, with the consequence that the richness and breadth that characterized the earlier volume is missing.

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JAMES E. CRONIN, editor. *The Diary of Elihu Hubbard Smith (1771-1798)*. (Memoirs of the American Philosophical Society, volume 95.) Philadelphia: the Society. 1973. Pp. xiii, 481. \$15.00.

Elihu Hubbard Smith was a bright young man of amazing energy and multifarious interests. His thoughts about activities in Connecticut and New York and involvement in medical and literary circles at the end of the eighteenth century merit scrutiny. Although he died shortly after his twenty-seventh birthday, Smith accurately, if a bit immodestly, describes himself as an individual whose "attainments certainly surpass those of most persons of my age . . . It is probable that the

same is true of my talents." In addition to obligations as a physician, he wrote an opera, published the first anthology of American poetry, joined the Connecticut Wits in a publishing venture, and served as friend and critic to dramatist William Dunlap and novelist Charles Brockden Brown. As a physician he tended patients and corresponded with figures such as Benjamin Rush and Benjamin Smith Barton. He also helped found the Hartford County Medical Society, and with Edward Miller and Samuel Latham Mitchell founded the *Medical Repository*, the first American medical journal. He was also a voracious reader, inveterate theatergoer, and compulsive writer. Though barely begun, Smith's life was already complex; he simultaneously involved his mind and efforts in numerous activities.

The diary includes not only entries spanning the period 1795 to 1798, but also weekly and monthly summaries, nearly two hundred letters, and several essays. Smith is adept with words, and his self-conscious literary effort is eminently quotable when characterizing people, probing philosophical or medical problems, evaluating dramatic and literary performances, or describing various social conditions. Smith's descriptions of living conditions and social habits lucidly recreate life. For example, his interest in fevers—a problem of intense public concern—and diseases frequently caused him to portray conditions of filth, dampness, and overcrowding which clearly destroyed health. When he casually records his own ill-health as he sets off to deliver babies or visit the sick, he unconsciously reveals a good deal as well. He provides useful descriptions of medical and scientific education and practices. Although Smith admitted that "we must look to Europe, for a long time, for a large portion of our scientific improvement," he also actively sought observations from qualified Americans. Knowledge about medicine, agriculture, indeed any useful knowledge, was to be shared, for the editors of *The Medical Repository* hoped to serve as "conduit-pipes, in conveying this information to the public." This young bachelor's opinions concerning family, child-rearing, sex roles, and the like are certainly fascinating. His family and kin remained important to him, and his correspondence and entries in the diary reveal significant details about family relationships. Particularly valuable in this regard are his lengthy and vivid recollections of his childhood. His fascination with sex roles and interest in Mary Wollstonecraft's writings crop up periodically.

We are indebted to James E. Cronin, not only for making this detailed diary available, but for sensible editing, which enhanced the work. A brief biographical sketch of Smith, a glossary of about fifty individuals, and terse yet illuminating foot-

notes are valuable additions. Cronin's rearrangement of certain sections to create a more coherent whole and recapture chronology strengthened the volume; omission of certain extraneous matter and material previously printed and available tightened it.

The diary certainly figured as a major and significant project to Smith, for during three years he devoted considerable time and effort to produce nearly half a million words in it. Smith thus expected to provide a valuable contribution to subsequent generations, and especially to historians. We are indebted to Cronin for capably preparing this legacy, which should enrich those interested in American studies, social and intellectual history, and medical history.

RANDOLPH S. KLEIN  
Connecticut College

JAMES THOMAS FLEXNER. *Washington: The Indispensable Man*. Boston: Little, Brown and Company. 1974. Pp. xvii, 423. \$12.50.

Scholars of the American Revolution era will quickly recognize this abridgment of Flexner's four-volume biography of Washington. Apparently designed as a bicentennial contribution for the general reader, the book attempts to capture the essence of the larger work without burdening the casual reader with unnecessary impedimenta such as footnotes and a comprehensive bibliography. Consequently, anyone seeking a full treatment of any aspect of Washington's life or documentation of information must still consult the earlier volumes.

Flexner attempts to accomplish three main objectives. First, he discards all previous accounts of Washington and returns to eighteenth-century sources for his information, certainly a laudable decision in view of the slanted biographies written by glorifiers and debunkers alike. Then, as the title indicates, he contends that Washington was indispensable to the success of the American Revolution, the Constitution, and the establishment of a viable republican government from 1789 to 1797. And finally, he attempts to put the real flesh and blood on the marble bust and the posed portraits that most of us associate with the first president.

The author succeeds in depicting the successes and failures of a man destined for immortality. Starting from a modest background of a grade-school education and a small planter family, Washington was among the most precocious of all noted Americans, was always a leader, and showed early signs of greatness by being Virginia's most celebrated hero at age twenty-two. Yet not all was success, for he fell in love with Sally Fairfax, the wife of his friend George William Fairfax,

married the widow Martha Custis, and then wrote at the end of his life that his happiest moments were those that he spent in Sally's presence. Although he was a hero to Virginians for his early military exploits, he did not impress the British and thus did not achieve the military fame in the British army that he desired. As a general in the Revolution, he failed when he copied British military tactics and succeeded only when he used men and terrain intelligently in a hit-and-run type of warfare. As president, Washington also experienced success and failure—success in getting the country started but failure in preserving the unity among leaders that he considered so essential. And finally, a man who was loved and respected by his own soldiers, who impressed Abigail Adams with his social graces and other women with his impudence (the kind that women liked), and whose wife called him "her old man" had to possess human characteristics in a large way.

Not all readers will be happy with this biography. Partisans of Jefferson and John Adams may feel that their men have not received just treatment; and, if biographers come either to love or to hate their subjects, no one will be in doubt about Flexner's position. Yet with all this, the book can be highly recommended for the general reader.

ROBERT E. BROWN  
Michigan State University

DUNCAN J. MACLEOD. *Slavery, Race and the American Revolution*. New York: Cambridge University Press. 1974. Pp. vi, 249. \$15.95.

"The American experience knows no greater tragedy than the old South's twistings and turnings on the rack of slavery." Charles Grier Sellers wrote those words about the antebellum South. Duncan MacLeod finds the crucial point in the development of that tragedy in the American Revolution. By expanding and intensifying debate over an institution already a century old, the Revolution's libertarian philosophy "laid bare" the anomaly of slavery in a society which championed freedom.

The fledgling antislavery movement embraced the Revolutionary philosophy, imbuing it with a religious character. To Quakers and Puritans, the sin of slavery embarrassed the nation abroad and corrupted it at home. But slavery, the author emphasizes, was not crucial to Northern economic or cultural life, as it was in the South. Equally attached to Republican principles and slavery, Southerners ultimately found it necessary to adapt those principles to the realities of human bondage.

Much of this is familiar ground; MacLeod's contribution is his careful examination of the process by which Southerners accommodated Revolutionary ideology to a slave society. It was a process

that led the South away from a "pure republicanism" into a legal and constitutional conservatism. It affected institutions such as the courts and political leaders who discovered that their economic and political interests demanded a personal accommodation to slavery. Tragically, the process reinforced racial prejudice because Southerners sought "in the character of the Negro the explanation for slavery which the Revolutionary debate demanded." Thus one legacy of the Revolution was a positive racism which explicitly denied the rights the Revolution proclaimed.

MacLeod's evidence linking the process of accommodation to Revolutionary ideology is not entirely persuasive. Nor can one easily accept his view—crucial to his argument that the Revolution was a watershed—that slavery became an avowedly racial institution only after the Revolution.

PETER M. MITCHELL  
Seton Hall University

contributes to a better understanding of the background of Anglo-American colonization in Texas, giving an alternative to the viewpoint on colonization offered by the Austins. The papers presented here deal primarily with the background to the empresario contract. Some material in the introduction does not bear directly upon the subject and may be considered antiquarian in nature, although it contains interesting and little-known morsels about the history of the American Southwest and Texas.

The publication is fully documented, contains several interesting maps, and has a complete and useful index. The index does not have adequate cross-references, however, and is cluttered with the names of places where printed items cited were published and with the names of depositories housing documents mentioned.

JOSEPH MILTON NANCE  
Texas A&M University

MALCOLM D. MCLEAN, compiled and edited by. *Papers Concerning Robertson's Colony in Texas*. Volume 1, 1788–1822. Fort Worth, Tex.: Texas Christian University Press. 1974. Pp. lxxi, 566. \$20.00.

This volume of the *Papers Concerning Robertson's Colony in Texas* is the first of one of the most significant and extensive collections (ultimately eight or nine volumes) pertaining to the early Anglo-American colonization of Mexican-Texas by the Texas Association. This small group of Tennessee and Kentucky promoters and land speculators were bent on taking advantage of the liberal Spanish-Mexican land policy in Texas. The group included Nelson Patteson, Sam Houston, John W. Overton, Andrew Erwin, Ira Ingram, John Harding, Amos Edwards, Robert Leftwich, and four sons, a grandson, and a nephew of North Carolina-Tennessee frontiersman and colonizer General James Robertson.

Much of the material in this volume appears in print for the first time and includes personal correspondence and official documents of various kinds that have been translated and edited by Malcolm D. McLean. Serious students of Texan history and of the westward movement in the United States will be forever grateful to the author and to the Kathryn O'Connor Foundation for making available important source materials so meticulously and accurately edited.

Volume I contains a lengthy, somewhat folksy, rambling introduction with a few unnecessary presumptions. It traces briefly the early Spanish and French expeditions and activities touching upon central Texas, supplies valuable biographical data on many individuals, contains an account of the preservation of the Robertson Colony Papers, and

DAVID P. MCKAY and RICHARD CRAWFORD. *William Billings of Boston: Eighteenth-Century Composer*. Princeton: Princeton University Press. 1975. Pp. xii, 303. \$16.00.

*William Billings of Boston*, based mainly on exhaustive research in primary sources, is unquestionably the long-awaited, definitive statement on "the foremost American musician of the eighteenth century" (p. ix). Billings may have lacked proper recognition in the past. But now McKay and Crawford place the Boston composer in the pantheon of America's culture heroes. No general study can hereafter ignore Billings, whose significance is so convincingly revealed.

Billings' unprepossessing appearance—deformed in person, blind in one eye, one leg shorter than the other, one arm somewhat withered—masked an "artist of the people," whose career paralleled his country's changing fortunes. His *Singing Master's Assistant* (1778) is "the most impressive published work by any American musician of the eighteenth century" (p. 77). Billings' rollicking prefaces—a prose of "comic ingenuity and verbal virtuosity" (p. 83)—mirror the swaggering self-confidence of revolutionary America. "I am not tied down by any rules laid down by others," he stated. He had strayed far from the Calvinist tradition of enforced musical simplicity. "O enchanting! O ecstatic!" wrote Billings in defense of the fuguing tune. "Push on, push on, ye sons of harmony . . ." (p. 177).

The authors place Billings' comic prose in a tradition of oratorical-literary resonance with François Rabelais, W. C. Fields, and Father Divine. If Henry Fielding were a possible prose model for Billings, the music itself has a more



archaic quality reflecting the isolation of Americans from the mainstream of European developments. American composers paralleled the medieval practice of the "successive" composition of the voices of a work rather than their "simultaneous" composition as in Palestrina and later composers. Billings' response, however, to the declamatory potential of his texts, according to the authors, recalls such masters as Monteverdi and Schütz.

"Father, compiler and publisher, composer, singing-teacher, and . . . self-declared *litterateur*" (p. 103), Billings was also a patriot. *Chester* and the *Lamentation Over Boston* "helped to focus the militant emotions the American Revolution aroused." "It is a truly valuable addition to America's symbols of democratic culture," remark the authors, "that a Boston tanner was our first native musical artist" (p. 189).

Introducing six chapters that combine narrative and analysis is a superb prologue, "Eighteenth-Century Sacred Music in New England," from which students of American culture will particularly profit. An epilogue interestingly surveys "The Reputation of Billings and his Music, 1800-1970." Numerous musical examples explicate the text. Appendices include important accounts of the inhibiting effect of copyright law, performance of Billings' music, and instructive illustrations.

THOMAS WENDEL  
*San Jose State University*

GEORGE DARGO. *Jefferson's Louisiana: Politics and the Clash of Legal Traditions*. (Studies in Legal History. Published in association with the American Society for Legal History.) Cambridge, Mass.: Harvard University Press. 1975. Pp. x, 260. \$15.00.

In 1803 Jefferson's Virginia had experienced 184 years of local self-government; Jefferson's Louisiana could remember only a century of colonial rule, barren of even the slightest acquaintance with the democratic process. The task of bringing this relict of European imperialism into the union of "sovereign American states" was inevitably laden with monumental difficulty.

This compact study is a long-needed and excellently crafted examination of the variety of levels on which the Latin culture and society of Louisiana attempted to preserve its identity and maintain a continuity with its past even as it assumed a new role in the dominantly Anglo-American republic. Because of his particular attachment to legal history, George Dargo is intrigued by the fierce battle waged in the territory of Orleans between protagonists of the civil- and common-law traditions, which in many ways symbolized the clash of cultures. He contends that the

adoption by the territorial legislature of the Civil Digest of 1808 and its acceptance by Jefferson and the American authorities represented a significant resolution of the struggle and one which helped guide national policy thereafter.

The great value of Dargo's presentation is in his realization that the legal battle encapsulated an infinitely complex confrontation and in the skill with which he demonstrates that the 1808 accommodation was more than simply a legal decision. He explores and connects to his theme such diverse developments as the Burr Conspiracy and Edward Livingston's clash with Jefferson over principles of law in the famous *Batture* controversy.

Dargo allows a few careless blunders concerning legal aspects of the Burr trial to slip into his pages, but this is a minor flaw in a first-class study.

JOSEPH G. TREGLE, JR.  
*University of New Orleans*

RENNARD STRICKLAND. *Fire and the Spirits: Cherokee Law from Clan to Court*. Foreword by NEILL H. ALFORD, JR. (The Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1975. Pp. xx, 260. \$9.95.

Rennard Strickland, a legal scholar and long-time student of the Cherokees, focuses on law as practiced by a people who apparently accepted the American system. He persuasively argues, however, that Cherokee law was not created from a dramatic cleavage with past "savagery." In fact, Strickland discovered that Anglo models were adapted slowly to existing tribal traditions—traditions that have remained among primitive Cherokee elements.

Viewing law as dynamic and organic, Strickland traces the development of the Cherokee legal system, its nurture in an Indian and mixed environment, and its adaptation to encroaching white civilization. Although the first written law was promulgated in 1808, the Cherokees already had a reverence for legality which persisted beyond the end of their judicial system in 1898. The idea behind the institution received attention in terms of the Cherokees' traditional law ways, their goals and values, and the development and adaptability of the codes. Strickland pushes Indian history beyond the simple narrative chronicle of events into the realms of institutional and theoretical studies.

Despite the merits of thorough research and promising insights in this work, some weaknesses stand out. Most obvious is the omission of probing examination of the crucial events in Cherokee legal history. Strickland gives only a cursory glance at the critical issue of Cherokee reunification in Indian Territory in 1839. He fails to explain how the



Cherokees managed to construct a new government in the face of bitter and bloody factionalism. Space given to copying Cherokee laws in their entirety might better have been allotted to their analysis.

Strickland's rigorous standards of scholarship will enhance this addition to the already widely acclaimed *Civilization of the American Indian Series*, and he is sure to find an appreciative audience among students of Indian and legal history.

GARY E. MOULTON

*Southwestern Oklahoma State University*

ROBERT L. ALEXANDER. *The Architecture of Maximilian Godefroy*. (The Johns Hopkins Studies in Nineteenth-Century Architecture.) Baltimore: Johns Hopkins University Press. 1974. Pp. 246. \$16.00.

In 1805, at the age of forty, Maximilian Godefroy, a civil engineer, came to the United States as a refugee from France and settled in Baltimore, where for twelve years he taught engineering and drawing at St. Mary's College. Self-educated in architecture, Godefroy was soon engaged in wide-ranging architectural activities, designing churches, banks, an exchange, a courthouse, monuments, burial vaults, and fortifications, though only one private house. A man of great personal charm, Godefroy was also erratic and temperamental, sometimes quarreling bitterly with those about him. When he fell on hard times in the panic of 1819, he suddenly left the United States for England. He remained in London for seven years and then returned to France to practice architecture in the provinces. He died about 1840.

Godefroy introduced into the United States the neoclassical forms and decorative details that he gleaned from French architectural books. Probably his most important American work was the Unitarian Church in Baltimore (1818), designed in an austere, boldly geometrical, classical style, which was popular in France a generation earlier. St. Mary's Chapel (1808) mixed Gothic elements with classical details. The Battle Monument (1822), perhaps the earliest civic monument in the United States, added Egyptian motifs to classical forms. His engineering work at Fort McHenry (1814) embodied ideas that he had previously set forth in a small book on military defenses appropriate to the United States. Godefroy's buildings, freely inventive and fusing a variety of stylistic elements, stood far outside the American mainstream of late federal and early Greek revival forms of the time.

Robert Alexander has labored mightily to bring together information on this little-known architect and his work. The footnote documentation is ex-

haustive. The extended discussion of conjectured prototypes of Godefroy's architectural ideas may seem excessive, however, to the historian with no specific interest in architecture. An inelegant prose style and the omission of a bibliography weaken the monograph.

PAUL R. BAKER

*New York University*

MICHAEL PAUL ROGIN. *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian*. New York: Alfred A. Knopf. 1975. Pp. 373, xii. \$13.95.

Michael Rogin has written a thought-provoking study of Andrew Jackson's role in American history as viewed through the perspectives of psychohistory, marketplace analysis, and Indian-white relations. He argues that historians have failed to recognize that Indians were at the very center of Jackson's life and the age named after him. Indeed, America was "built on Indian graves." The replacement of Indians by whites on the expanding frontier symbolized America's maturation from childhood, its regeneration through violence, and its transfer "from nature to capitalist civilization."

In his encounter with the Indians, Jackson confronted not only another culture but also his own fantasies, longings, and fears. The subjugation of Indians helped him to "destroy lingering fears of feminine domination and grow securely to manhood." The familial language he employed in speaking to and about Indians, Rogin claims, had more than symbolic meaning. Jackson's entire life was allegedly a continual battle to establish "paternal authority." His generation contrasted its own materialism unfavorably with the heroism of the revolutionary fathers. "Too young" to fight in the Revolution, arriving "too late" in Tennessee to share the glory of James Robertson and John Sevier, suffering from "anxieties over bodily control," and uncertain whether he had actually "imbibed courage with his mother's milk," Jackson sought paternal authority through "just war" against the Creeks and later the Seminoles. His defeat of these Indians "emancipated him from parental domination" and established him in the ranks of the fathers. The major formulas of Jacksonian Democracy also developed in Jackson's dealings with the Indians. His new paternalism supposedly explains the war on "the Mother Bank" and other "Jacksonian reforms."

Although this is a fascinating book, it suffers from several serious defects. Rogin qualifies many of his statements with a "perhaps" or a "may have." Yet he ignores these qualifiers when he draws his conclusions and, thereby, forecloses al-

ternative hypotheses. There are serious problems involved in examining the human psyche to achieve a comprehensive analysis of motivation when the subject is dead and only traces of his personality can be analyzed. Not only is there no one accepted theory of human motivation today, but even the demonstrated existence of a recognizable pathology may not necessarily explain all of an individual's actions. Such problems relating to the writing of psychobiography aside, the book also suffers from some factual errors and problems of interpretation, including sweeping generalizations that lack scholarly documentation. For example, Rogin's statements about Jackson's hatred of the Indians, and his defiance of the Supreme Court's decision in *Worcester v. Georgia*, among others, are refuted in my *American Indian Policy in the Jacksonian Era*. Psychohistorians will undoubtedly applaud the appearance of this book, but historians of Indian-white relations will find the author's tendency to overlook the frontier antecedents of many of Jackson's policies a major weakness. In spite of such problems, however, Rogin's attempt to penetrate into "the dark interior of Jackson's life and career" is interesting reading.

RONALD N. SATZ  
University of Tennessee,  
Martin

JOHN MICHAEL CUDD. *The Chicopee Manufacturing Company, 1823-1915*. Foreword by SIDNEY RATNER. Wilmington, Del.: Scholarly Resources Inc. 1975. Pp. xiv, 325. \$8.95.

This company history suffers from the limited nature of its primary sources. These consist chiefly of directors' and stockholders' minutes spanning the years 1823-1837 and a complete run of semiannual, annual, and special reports issued by Chicopee's chief executive, the treasurer, for the latter half of its history (1861-1915). On the basis of these, Cudd analyzes early corporate formation and structure and quantifies costs, output, sales, and profits from the Civil War onwards. Chicopee, a vertically integrated, cotton-manufacturing company, emerges in its antebellum years as one of the less-successful promotions of the Boston Associates. It was most profitable in the decade after the Civil War when three-quarters of production consisted of flannels. Then its dividends were consistently higher than the average found in a sample of New England cotton companies. In 1871 it declared its highest dividend, seventy percent. The three decades after 1885 saw the company's relative position decline, until in 1915 it was purchased by Johnson & Johnson to secure their wartime supply of medical gauze.

Because of the nature of the sources, we are

denied precise insights into the changing problems of production and marketing. To bridge the hiatuses, Cudd conjectures from the economic and technological performances of comparable textile companies studied by other business historians. This leads him into suppositions which at times are dubious. For example, estimates of cloth production from spindleage assume the constancy of at least nine technological and three economic variables. Additionally, Cudd could have explored the backgrounds of the first stockholders, directors, and mill managers with greater tenacity.

In short, this volume offers an externalized, and in parts protracted, view of the Chicopee Manufacturing Company's development. It is a useful, but hardly pathbreaking, study.

DAVID J. JEREMY  
Westcliff-on-Sea, England

LAWRENCE J. FRIEDMAN. *Inventors of the Promised Land*. New York: Alfred A. Knopf. 1975. Pp. xviii, 344. xiii. \$15.00.

According to Lawrence J. Friedman, the "spread-eagle" patriotism of the early national and Jacksonian periods was permeated by sexism in the form of the ideal of "True American Womanhood" and racism as reflected in a widespread commitment to the removal of blacks from the United States. The vision of "the rising glory of America" was thus a constricting, diversity-denying image of white male supremacy. Friedman sees this narrow and bigoted patriotism as the outgrowth of an effort to combine "rootedness" or stability with a continual striving to live up to the ideal of individual and national perfection established by the revolutionary generation. The tensions created by this contradictory quest impelled patriots to reject equality for women and blacks and to place a wall of rhetoric between themselves and the complex realities of human existence.

There is clearly a measure of truth in Friedman's thesis. What we today call racism and sexism were obviously prevalent American attitudes during this period and presumably influenced the quest for an American nationality. But Friedman weakens his own case by trying to force too many individuals and movements into the same bag. As he sees it, every patriot was a narrow chauvinist, and even those white male reformers who sought enlarged opportunities for blacks and women fully shared the general commitment to white male supremacy. To sustain this image of a malevolent consensus, he engages in a number of questionable practices. His selection of individuals, incidents, and quotations to be discussed is even more arbitrary than is normally the case with intellectual historians, and his attempts at psychological pene-

tration seem loose or strained even by the flexible standards of psychohistory.

Friedman loads the dice by limiting his discussion of the early prophets of American nationality almost entirely to the Federalist or conservative school of thought. His pretext for ignoring the Jeffersonians is that the sage of Monticello was an "intellectual," who recognized that the patriotic crusade "was antithetical to the life of the mind." Yet Jefferson certainly considered himself a patriot and had strong views about American mission. Furthermore, in his role as a major political leader he presumably spoke for the hopes and aspirations of a large number of his countrymen. Friedman ignores the possibility that the Jeffersonian view of America as a testing ground for Enlightenment ideas of liberty and equality might have competed with the particularistic nationalism of Federalists. The real issue is why the Enlightenment promise of liberty and equality was not extended to blacks and women. But Friedman's commitment to demonstrating that American patriotic values were rotten to the core will not allow him to see the question in this light.

Even more lamentable is Friedman's misguided effort to show that Garrison and other radical abolitionists remained covert colonizationists and proponents of an exclusive white nationality even after they had rejected the American Colonization Society. His method is to seize upon a few isolated and tentative statements of sympathy for limited black-emigration projects. It seems incredible that anyone could actually read through Garrison's writings with their continual and eloquent insistence on interracial fraternity and conclude that he was a white nationalist at heart. When Friedman attempts to include in his colonizationist consensus the slavocrats of the deep South—men whose very identity depended on the presence rather than the absence of blacks—Friedman's bag simply bursts. This book began with a modicum of truth, but then obscured it by exaggeration and excessive speculation.

GEORGE M. FREDRICKSON  
Northwestern University

STEPHEN B. OATES. *The Fires of Jubilee: Nat Turner's Fierce Rebellion*. New York: Harper and Row. 1975. Pp. xiii, 187. \$7.95.

Stephen B. Oates, of the University of Massachusetts, is perhaps best known for his good biography of John Brown, which appeared in 1970. This account of the Turner uprising in Virginia in 1831 is generally accurate and is filled out with imaginative suggestions which seem sensible. More might well have been done on the situation in the South during the decade prior to the outbreak and on the

machinery of control which confronted would-be insurrectionists. The impact of the rebellion is well done, and here some new material—especially from the unpublished diary of Governor Floyd of Virginia—is brought forward.

Oates' account differs markedly from that in William Styron's best-selling novel of 1967 on the same subject; it confirms the findings in Eric Foner's reader of 1971, the splendid documentary edited by Henry I. Tragle, also appearing in 1971, and the work on the subject by this reviewer, written some forty years ago.

HERBERT APTHEKER  
Hostos Community College  
City University of New York

RAYMOND A. SCHROTH, S. J. *The Eagle and Brooklyn: A Community Newspaper, 1841-1955*. (Contributions in American Studies, number 13.) Westport, Conn.: Greenwood Press. 1974. Pp. xv, 304. \$12.95.

As its title suggests, this is not just the history of a newspaper, but a study of the relationships and interactions between that newspaper and the community in which it was published for more than a century. *The Eagle and Brooklyn* provide a particularly apt combination for such a study.

Despite being overshadowed by the newspaper giants in nearby Manhattan, the *Eagle* was for many years one of the more highly regarded American journals. Under the editorship of men like Walt Whitman and St. Clair McKelway, it earned an international reputation. Yet it was essentially a provincial newspaper, its fortunes closely intertwined with those of Brooklyn. The *Eagle* declined with the city and died in 1955, the immediate casualty of a long and bitter fight between its owners and the American Newspaper Guild, which culminated in a strike in January of that year.

This is greatly to oversimplify the many-faceted story of the life and death of a newspaper, which, in attempting to cling to the patterns of operation that had made it a success, could not adjust to the startling changes that were taking place around it. Its editors and publishers remained committed to a mystical concept of "the Brooklyn spirit" long after economic and demographic forces had tied it more closely to Manhattan. These same forces opened up a new potential for the *Eagle* on Long Island which was largely ignored.

The author—a nephew of the *Eagle's* last publisher, Frank D. Schroth—provides a lively, if necessarily foreshortened, account of the growth and decline of a newspaper, while appearing to avoid any bias on the side of his uncle and other members of the Schroth family. His analysis of the relationships between the *Eagle* and Brooklyn are always perceptive, sometimes striking.

More studies of this sort would greatly enhance the history of journalism in the United States, which has been written almost exclusively in terms of its "giants," and with little regard for the kinds of interactions with their communities that give an added dimension to *The Eagle and Brooklyn*.

JOHN M. HARRISON  
*Pennsylvania State University*

BRUCE SINCLAIR. *Philadelphia's Philosopher Mechanics: A History of the Franklin Institute 1824-1865*. (History of Technology.) Baltimore: Johns Hopkins University Press. 1975. Pp. xi, 353. \$15.00.

ELTING E. MORISON. *From Know-How to Nowhere: The Development of American Technology*. New York: Basic Books. 1975. Pp. xiii, 199. \$10.00.

Bruce Sinclair traces the history of the Franklin Institute of Philadelphia from its founding in 1824 as a mechanics' institute to its emergence as a very different kind of institution in 1865. Two themes run through his account. The first concerns the relation between science and technology. The founders of the Institute were convinced that the two should be brought together. They discovered slowly and painfully, however, that the process did not involve using scientific theories to enhance technology. Instead it meant applying the experimental methods and the intellectual rigor of science to technology. The Institute provided a forum in which Philadelphia's scientific community and its emerging industrialists could work out this new and useful relationship. The theme pervades almost all aspects of the Institute's activities—its important publication (the *Journal of the Franklin Institute*), its varied educational endeavors, and its scientific and technical research.

Sinclair's second theme concerns the problems of a scientific and technical institution in the rapidly changing decades of the pre-Civil War years. The Institute showed a considerable ability to adapt itself to changing circumstances and clients. Originally a mechanics' institute, it was later transformed by Alexander Dallas Bache and others into a national center for technical research and technical adviser to both state and national governments. By the time of the Civil War, however, the Institute moved in yet another direction as it sought to meet the needs of Philadelphia's engineering industries.

This is a first-rate study that demonstrates how the analysis of a local scientific institution can illuminate larger national issues. The research is detailed and thorough; the writing is clear and consistently holds the reader's interest. If the price seems high, the reader can be assured that he receives not only a thorough bibliography and a good index but also such unusual amenities as

twenty-six pages of illustrations and elaborate footnotes at the bottom of each page.

If Sinclair illuminates larger issues by analyzing a local scientific institution, Elting E. Morison attacks the "big questions" directly, for in his book the history of American technology becomes part of a prophetic vision. Morison agrees with Alfred North Whitehead that the business of "philosophers, students, and practical men" is "to recreate and reenact a vision of the world." Either "we must succeed in providing a rational coordination of impulses and thoughts, or for centuries civilization will sink into a mere welter of minor excitements" (pp. 14-15). It is to this task that Morison puts his very considerable analytical powers.

His argument is straightforward: man has developed a scientific and technological method which enables him to create technical systems that "in their operation profoundly modify and sometimes take the place of our natural environment" (p. 3). Technology has developed an internal logic of its own which has generated change that has destroyed the idea of progress, the governing vision of the industrializing world, and has brought into question the ends that technology presumably serves.

Morison suggests a new vision for a technological society. That vision is thoroughly humanistic, for the central assumption underlying it is that the "technological universe should be designed to fit and serve the human dimensions" (p. 169). His method similarly owes more to the humanities than to science. He writes of "visions" and titles his penultimate chapter "The Parable of the Ships at Sea." Although he reports that a scientist friend "finds my idea that the narrative can be a form of thought hilarious" (p. ix), Morison depends on that form of historical discourse in the bulk of his volume.

The heart of the work is a series of related essays on facets of the history of American technology ranging from the builders of the Middlesex Canal to the growing importance of scientific knowledge, particularly as displayed in the development of the incandescent electric lamp at the General Electric Research Laboratory. A person reasonably well read in the history of American technology will find little new information in these chapters. But the essays are filled with lucid exposition of the changing modes of thought that characterized engineering in America and offer an impressive discussion of the growing interaction between science and technology in the twentieth century. Furthermore, the argument is presented in urbane prose that makes the book a delight to read.

The two books share at least one common concern: the relationship between science and tech-



nology. Both would probably agree that, as Morison put it, in the pre-Civil War period science offered few "general ideas or theoretical considerations that would help in building and making new things" (p. 90). While Morison is concerned with the provision of scientific theoretical underpinnings for technology in subsequent decades, Sinclair emphasizes the ways in which science's experimental method, intellectual rigor, and emphasis on original research had begun to influence Philadelphia industrialists before 1860. This is a topic that deserves much more research, but both books represent a significant contribution to the topic and to the general history of American technology.

KENDALL BIRR  
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Albany

MERTON L. DILLON. *The Abolitionists: The Growth of a Dissenting Minority*. (Minorities of American History.) DeKalb: Northern Illinois University Press. 1974. Pp. xiii, 298.

Like oil wells, historical topics sometimes run dry. In this judicious study Merton Dillon, author of two fine antislavery biographies and several classic articles in the field, surveys the abolitionist experience from the American Revolution through the Civil War. Nevertheless, the procedure is a bit arid. Gilbert Barnes, Dwight Dumond, Louis Filler, Gerald Sorin, and others have traversed the same terrain with varying degrees of success. Dillon's contribution is no less worthy than theirs, and his mixture of sympathy and critical judgment provides a better balance than most other examples of the genre. Yet the book comes late in the day.

Dillon graciously acknowledges the work of recent scholars—Kraditor, Stewart, Thomas, Perry, the Peases, which he skillfully weaves into his organization; unfortunately, though, he does not challenge their sometimes contradictory views or move the topic into new directions. By and large, this work is a conventional narrative about the rise and impact of antislavery, chiefly in its political rather than intellectual phases. Given the author's undoubted knowledge of the complexities involved in abolitionist reform, the result is bland and homogenized. Not until the final pages does he raise a suggestive question: just how important were the abolitionists in the overthrow of slavery? Less than we might think, he replies, because, as he remarks earlier, "something beyond moral commitment and skill was needed to assure abolitionist success" (p. 83). Southern foolhardiness and volatility did more to stimulate anti-Southern reactions than

the actions of reformers alone. It may be an old perception, but he could have pursued it with greater vigor and precision than his predecessors did. Otherwise, Dillon hugs pretty close to familiar pathways. One would have liked to see less about Garrison and Tappan, more about antislavery lawyers who ingeniously forced the legal system to confront constitutional questions in the fugitive and personal liberty cases; less about the 1840 division and more about the sectional church schisms—in the mode of Donald Mathews; less about reform leaders in general, more about followers, about whom we still know little. As a straightforward, narrative survey, the work is certainly satisfactory, but as a provocative interpretation of antislavery significance, it is a mild disappointment.

BERTRAM WYATT-BROWN  
Case Western Reserve University

DAVID C. KLINGAMAN and RICHARD K. VEDDER, editors. *Essays in Nineteenth-Century Economic History: The Old Northwest*. Athens, Ohio: Ohio University Press. 1975. Pp. xiv, 356. \$12.00.

Viewed from a charitable perspective, this collection of eleven essays combines the talents of five seasoned scholars in economic history with the work of some newer contributors. This is a book of highly uneven quality, but there are some gems to be found in it. And it is produced handsomely. But viewed from another angle, it is astonishing that its publication can be justified at a time when university presses are constantly turning down excellent manuscripts of young scholars because of fiscal constraints. The longest of the contributions—an eighty-seven page essay on railroads and economic development from 1870 to 1890 by Jeffrey G. Williamson—consists of verbatim sections of Williamson's book, *Late Nineteenth-Century American Development* (1974). Another essay is merely a recapitulation of Roger Ransom's well-known published articles on social returns from canal investment. And I think two of the remaining chapters could never have passed muster with referees of even a middling-quality historical or economics journal.

What, then, of the other half of the book? The most valuable contributions here, apart from Williamson's, are by Robert E. Gallman and Richard A. Easterlin. It speaks poorly for the precision of the book's title that neither of these two essays treats principally the Old Northwest; both deal with agricultural output, income, labor force, and productivity for the nation as a whole in the nineteenth century. As one would expect of his work, Gallman's study reveals ingenuity and remarkable



industry in the gathering and analysis of data, together with rigorous presentation of quantitative measures from the prestatistical era. Gallman reconsiders earlier estimates (by Paul David and others), and he argues persuasively that agricultural productivity accelerated greatly after 1850. He offers a startling partial explanation: there was under-utilization of farm labor in the early nineteenth century. The hours of farm labor, he maintains, were possibly one-third less than the hours of contemporary manufacturing workers; there were many slack periods and "substantial leisure."

Easterlin's study provides invaluable estimates of output, labor force, and income of farms by state for 1840 and by region for 1840-1860. He addresses the intriguing problem of "start-up" low-income periods in the early development of what later became rich farming regions. Also considered is the question of productivity on Southern farms and plantations; Easterlin argues that the South did not compare unfavorably with other regions. Finally, he contends that productivity gains attributable to shifts to more fertile western lands were probably less important, at ca. 5.6 percent from 1800 to 1840, than other sources, which totaled about 30 percent.

David Klingaman deals with changes in wealth in three northeastern Ohio counties, in an essay that, despite its severely localized data base, links nicely with one by Lee Soltow on the growth of wealth in Ohio. The latter study leans heavily on the weak reed of tax valuation data from the early nineteenth century. An interesting essay by Don R. Leet argues that the decline in birth rates preceded industrialization and urbanization in early Ohio, a pattern largely explained by rising population-to-land ratios and consequent declining opportunity. But lacking in Leet's analysis is any consideration of alternative opportunities to life and work on local farms, or their effects on fertility rates.

Richard Vedder and Lowell Gallaway contribute a workmanlike statistical study of migration and the Old Northwest, long on interesting data but short on analysis. The other essays—on Old Northwest regionalism, on banking, and on land speculation—offer little that is novel to students of these subjects. Presumably Williamson's study will be evaluated, as it should be, in reviews of his own book—undoubtedly one of the most challenging works in the literature of American economic history, but hardly one that required partial reprinting in this format.

HARRY N. SCHEIBER  
University of California,  
San Diego

WARD E. Y. ELLIOTT. *The Rise of Guardian Democracy: The Supreme Court's Role in Voting Rights Disputes, 1845-1969*. (Harvard Political Studies. Published under the direction of the Department of Government in Harvard University.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xiv, 391. \$15.00.

ROBERT M. COVER. *Justice Accused: Antislavery and the Judicial Process*. New Haven: Yale University Press. 1975. Pp. xii, 322. \$15.00.

Whether inspiring or exacerbating, high-court decisions between 1954 and 1974 helped to shape the period. In turn, campus disagreements about public policies shape scholars' views of the more distant past. Most historians try to minimize this presentism. The authors of both books under review approach their subjects in candidly present-minded ways.

Ward E. Y. Elliott, a political scientist, has grappled with aspects of the history of Supreme Court decisions involving voting rights. It is unfortunate that he expended his large talents to prepare a book-length missile against many of his co-disciplinarians.

His argument appears to be that academic theorists, including some in judicial robes, were—and are—ill-equipped to solve democracy's problems. Elliott deplores the Court judgments favoring swift social changes rather than acceptable progress. He charges that the Court assumed a guardian responsibility which it could not sustain and which proved to be seriously out of phase with public opinion and with minorities' true needs. Elliott wants judges to be restrained priests, not crusading prophets.

Whatever the merits of Elliott's dour view of the Kennedy-Johnson-Nixon years, the *Rise* portions of his *Guardian Democracy* lessen the book's impact. Elliott's historical data fail to support his judgment that alternative policy institutions were ready, willing, and able to act if courts and judges did not, or that inaction was preferable to judicial intervention. As examples, Conron's and Gettleman's work on the Dorr Rebellion and *Luther v. Borden* raise questions concerning legislative sensitivity to which Elliott does not address himself. His Civil War and Reconstruction analysis depends on publications no more recent than the very early 1960s. A fair share of the rich newer work directly disagrees with Elliott.

History offers sharp alternatives to the author's convictions that only the representative branches of government should initiate or shape social changes. The Court's history suggests that the high bench should and can balance as well as check. And better balances were what the voting rights cases were about.

Robert M. Cover is a lawyer. He wrote *Justice Accused* out of Vietnam-inspired concerns about whether judges should express distaste for allegedly undesirable public policies or simply render judgments based on existing laws. Cover's compelling illustration of the judge's conflict between duty and conscience is Captain Vere in Melville's *Billy Budd*.

Cover's is a better and more useful book than Elliott's; its tone is calm and its logic and documentation are impressive. This reviewer is less sure than Cover that "Judges . . . are conscious of the baggage of the past" (p. 7). With notable exceptions, I find today's legal professionals disarmingly ahistorical. But scholars should be grateful to Cover for his often brilliant illumination of tensions created in judges by changing eighteenth- and nineteenth-century jurisprudential attitudes and legal standards.

Cover suggests that the narrowly positivistic Captain Vere-role that even antislavery judges imposed on themselves, helped produce the 1860 deadlock in democracy's representative institutions, and that this attitude carried over into Reconstruction with, as this reviewer would add, equally corrosive effects. Therefore, the advantage in litigations lay with slaveowners and amorlists before 1865 and with opponents to race equality after Appomattox. With respect to race most nineteenth-century jurists remained priests not prophets.

Cover rarely descends to professional jargon, even when dealing with complex, choice-of-law conflicts, comity situations, and legal codification. I remain unsure, however, why Cover believes that Warren (not William) Howard's *American Slavers* is "confused" (p. 286), why he asserts (p. 166 n.) that *Ableman v. Booth* rather than *Tarble's Case* should remain the "leading" one on its central question (the whole Civil War, including the 1863 Habeas Corpus and the 1866 Civil Rights Acts, separated the two cases), and why, considering the author's presentism, the ten Broek-Graham view of the Fourteenth Amendment is less rigorous, because presentist (p. 155).

Cover should have caught the variants on the Ohio town (p. 185) and on Bertram Wyatt-Brown's name (p. 287), as well as several distracting typos (pp. 98, 171, and 285). Dallin Oaks' *University of Chicago Law Review* article is not the "only discussion in the legal literature of the writ *de homine repligando*" (p. 298); Catherine Tarrant deserves space on that uncrowded stage.

Cover's merits overshadow these shortcomings. He began this exciting adventure in interdisciplinary history with an apt reference to *Antigone*. This estimate of his and Elliott's book can end with another quotation from that useful model

of civil disobedience, where Creon says: "There is no art that teaches us to know/The temper, mind, or spirit of any man/Until he has been proved by government/And law giving."

HAROLD M. HYMAN  
Rice University

THOMAS BENDER. *Toward an Urban Vision: Ideas and Institutions in Nineteenth-Century America*. (Published for the Organization of American Historians.) Lexington: University Press of Kentucky. 1975. Pp. xv, 277. \$14.50.

This stimulating book deals with one small part of the historical development of the theory of urban society. During the early nineteenth century the new textile machinery led to the building of manufacturing towns whose growth from scratch provides the resourceful historian with excellent opportunities to study the reactions to urbanism of participants and observers under relatively simplified conditions. The intellectual environment in which this urban industrial development occurred was highly influenced by romanticism with its preoccupation with the esthetic and moral values of nature. The city represented a challenge to these values and required those Americans who were unwilling to repudiate urbanism to attempt to reconcile city and country. The most obvious solution was to bring the country into town in the form of parks, the inspiration for which came from the earlier rural cemetery movement. Lowell, Massachusetts, a result of the narrowly conceived manufacturing plans of the Boston capitalists, provides the illustrative material for the processes of urban growth.

By mid-century, Lowell had ceased to be a seven-days' wonder and had come to exhibit the perennial urban problems of poverty, crime, juvenile delinquency, and class conflict; and neither its spokesmen nor contemporary observers seem to have been able to articulate its problems in a useful manner. The author now shifts his attention rather abruptly to New York City, where Charles Loring Brace and Frederick Law Olmsted continued to explore the possibilities of the romantic reconciliation of nature and culture. Brace struggled vainly to preserve the values of community by strengthening the primary-group relationship, even to the extent of rustivating homeless and delinquent youths in rural midwestern homes. Olmsted seems to have persisted to the end of his life in the quaint conviction that city parks would elevate and humanize their users while reconciling the divergent interests of social classes.

Thomas Bender considers this movement a lost historical tradition alternative to the dominant

pattern of urban thought, whatever that may be. But the environmentalism of this lost tradition is certainly compatible with the ecological approach to urban study, which must be one of the major modes of dealing with urban development.

STOW PERSONS  
University of Iowa

IRA BERLIN. *Slaves without Masters: The Free Negro in the Antebellum South*. New York: Pantheon Books. 1974. Pp. xxi, 423. \$15.00.

*Slaves without Masters* is the first comprehensive study of Southern free Negroes before the Civil War. Ira Berlin's volume is a synthesis, drawing upon secondary materials as well as the author's own considerable research. This book is to some extent, however, a premature one. The field needs at present a large number of microanalytic local and county studies of free Negroes, utilizing such sources as census and tax records, local newspapers, and municipal archives. In the absence of these studies, Berlin necessarily must make frequent generalizations based on fragmentary evidence.

A good deal of the content of *Slaves without Masters* is to be found in the monographs of John Hope Franklin on North Carolina, Luther Porter Jackson on Virginia, and Donald Everett on Louisiana, among others. Furthermore, in Berlin's effort to flesh out his materials, he devotes an inordinate amount of space to his secondary theme—the nature of Southern race relations. For example, there is a great deal of unnecessary material on colonization.

Yet the book will attract a wide readership, both because of its overall picture of the status of free blacks through time, and because of the arresting contrast it draws between the free people of color of the upper South and those of the lower South. Berlin very soundly points out that the free Negro population of the deep South was much smaller and lighter skinned than the free blacks of the upper South. Only in the latter area had there been a substantial number of slaves manumitted during the era of the Revolution when a minority of slaveholders, motivated by the egalitarian ideology, freed a number of slaves who were not related to them by kinship ties. Yet paradoxically, although the deep South was more extreme in its proslavery ideology, the tiny group of free people of color living there was better educated, wealthier, and more skilled and enjoyed closer relations with aristocratic whites than their counterparts in the upper South. Berlin may overdraw the con-

trast, but he does put his finger on a very important matter.

Berlin's volume should stimulate other scholars to further in-depth research—particularly local studies of the antebellum free Negroes.

ELLIOTT RUDWICK  
Kent State University

JOHN F. COLEMAN. *The Disruption of the Pennsylvania Democracy, 1848-1860*. Harrisburg: Pennsylvania Historical and Museum Commission. 1975. Pp. v, 184. \$5.50.

Pennsylvania was a complex state with wide economic, religious, and ethnic diversities, but since 1800 it had evinced an astounding fidelity to the parties of Jefferson and Jackson in its politics. Overwhelmingly agricultural, Pennsylvania was a banner industrial state, and its economic interests were seemingly at variance with those of the party to which it clung so faithfully. John F. Coleman notes that "adroit party leadership, the adhesive of the patronage, the disarray of the opposition, and, above all, the weight of tradition kept Pennsylvania in the Democratic ranks" (p. 14).

From 1848 to 1860 the state experienced a political revolution which overthrew the Democrats and ensconced the new Republican party firmly in power. Political success shifted rapidly under the impact of such issues as the tariff, nativism, temperance, the ten-hour day, and, most of all, Free-Soilism. The Whig party enjoyed a brief ascendancy in 1848 but quickly disappeared through its own disunity. The Know-Nothings surged forward in 1854, but then declined, although they remained a potent political factor throughout the period. Though the Democrats experienced mixed success on the issues, they suffered constantly and severely from the factional disputes of the Dallasites, Buchananites, and Cameronians. The Republicans achieved success by uniting the opposition and emphasizing "economic self-interest, nativist principles, and hostility to the further spread of slavery" (p. 148).

This is a welcome addition to the Pennsylvania Historical and Museum Commission's series on Pennsylvania politics. After two introductory chapters Coleman proceeds through seven narrative chapters to his conclusion and provides twelve appendices, mainly devoted to election statistics. He writes well and evinces a thorough command of his complicated subject. Unfortunately, a short review cannot present more of his conclusions, and regrettably the volume is so brief that these conclusions often lack desirable supporting detail.

SANFORD W. HIGGINBOTHAM  
Rice University

PAUL H. MATTINGLY. *The Classless Profession: American Schoolmen in the Nineteenth Century*. (New York University Series in Education and Socialization in American History.) New York: New York University Press. 1975. Pp. xxiii, 235. \$15.00.

In this essay on nineteenth-century school leaders, Paul Mattingly reveals one of the ways that scholars have been writing history during the last decade. Compare this book, centered on the activities of the American Institute of Instruction from 1830 to 1918, to Richard Sennett's essays that took off from an analysis of family patterns in Chicago. The writer in each case began by selecting a group of people whose origins, career lines, and accomplishments could be laid out in analytical, quantitative form. The formal data produced mildly interesting information, but seem to have disappointed the writer himself, who then took off to exploit the subject as a way to exercise sensibility upon the minds of the people involved. With an organization like the AI, this intellectual tactic has considerable potential. The Institute began as a forum, which met in Boston, for men of professional, academic, or mildly patrician background, interested in promoting school reform. Over the years it included more working principals and administrators, and by the end of the century it turned into a slightly frivolous excuse for resort excursions.

Mattingly has concentrated on the ideas of the earlier leaders, drawing in material about teachers' institutes and other school endeavors. He shows these people talking about the need for teacher preparation, yet settling for a sense of that preparation as inspiration, revivalistic rousing, or the production of teachers with "character." In studying this nebulous rhetoric, he reveals their failure to perceive themselves in relation to either practical techniques or class identity. Referring to them as "schoolmen," he lets that term suggest that they were somehow representative of teachers in general. This still attributes too much importance to the rhetoric and too little to any function that the rhetoric played for people within a changing social structure. *The Classless Profession* pictures an elaborately textured mental surface. Did that surface, and its exclusion of technique, serve the same functions as does the culture of sensibility in the late twentieth century?

DANIEL CALHOUN  
*University of California,  
Davis*

FREDERICK M. BINDER. *The Age of the Common School, 1830-1865*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. ix, 191. \$8.95.

PATRICIA ALBJERG GRAHAM. *Community and Class in American Education, 1865-1918*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. xi, 256. \$8.95.

DAVID MADSEN. *Early National Education: 1776-1830*. (Studies in the History of American Education Series.) New York: John Wiley and Sons. 1974. Pp. vii, 162. \$8.95.

The history of American education has unfortunately been dropped as a requirement in many teacher-training programs at the time when the subject enjoys an emancipation from the narrow institutional and didactic constraints placed upon it by some of its founders. For more than a decade—largely through the work of Bernard Bailyn, Lawrence Cremin, Merle Borrowman, and their students—this field of study has outgrown its previous mission to document and celebrate the rise and triumph of both the American public school and our democratic form of government. Recently a group of younger revisionist historians set out to construct a radically critical interpretation, arguing that our schools have been the institutional means for inculcating middle-class values, which kept the burgeoning urban and immigrant proletariat law-abiding citizens and dependable laborers.

Taking advantage of the renewed interest in this subject, the editors of the series in which these three titles appear had hoped to provide "new interpretations of American educational history based on the best recent scholarship." Judging from these three books they have been only partially successful. The works range from an uninteresting restatement of old material to a fresh attempt at understanding the complexity of the relationship between schooling and the culture in which it is imbedded.

Frederick Binder's work on the common schools contains little that is new. Some attempt is made to counteract the excessive attention given to the New England reformers at the expense of leaders in other parts of the nation. He has also included an informative section on the elitist image that the first public high schools had in the minds of the wage earners who opposed them. This aside, his general thesis assumes that the public schools have been the most appropriate educational institutions for our democracy. Following this line of reasoning, he produces the thumb-worm album of heroes such as Horace Mann, Frederick Barnard, and leaders in the workingmen's societies along with the unflattering portraits of the unenlightened and perverse who differed with common-school reformers in approaches—such as the Boston Schoolmasters—or simply preferred private, religious, or familial alternatives—such as the officers of the American Sunday School Union.



Patricia Graham's work is in an entirely different mode. In place of a more general survey, covering a half-century of developments following the Civil War, she has studied four communities and two universities in depth and provides the reader with local case studies on the establishment and extension of comprehensive systems of schooling. Although such research is needed, the sum of the parts fails to make a whole. The fault here lies in her failure to state at the outset a clear rationale for her research. Not until the end are we informed why counties in Indiana, Michigan, and Alabama and the city of New York were singled out for study. In addition, the comparison between the changes at Princeton and those at the University of Wisconsin appears as an addendum on higher education rather than an integral part of the previous studies. This lack of a lucid basis is symptomatic of a larger problem, which is not satisfactorily solved by the author. Although her facts and statistics are impressive, her work lacks a clear conceptual framework that would order the data and make them relevant. What unifying theme there is again must be found at the end of the book. There she informs us in almost truistic terms that "where one lived was a fundamental determinant of one's educational opportunities."

Of the three authors, David Madsen has been the most successful in setting up a framework that makes the educational developments between 1776 and 1831 more understandable. Attempting to escape the narrow institutionalism, which has in the past choked off a more comprehensive educational history, he first turns to the political and cultural life of the new nation. This enables him to describe how much of a child's learning was familial and communal and, therefore, relatively unstructured and informal. This framework also reveals a basic social fabric upon which a number of educational planners attempted to superimpose their various systems of formal schooling. Once recognizing how intimately the mores of the postrevolutionary period were interwoven in the agrarian society of small landowners, one can see why Jefferson's "enlightened farmers" supported voluntary local arrangements—from district schools to academies—in preference to the national school system advocated by some of the republican intelligentsia. Because Madsen understands the life of the people, he makes a convincing case for why they educated their children as they did.

According to the scholarship of Ellwood P. Cubberley and Paul Monroe, the public schools have liberated our people from ignorance and enabled them to enjoy the greatest degree of equality of opportunity a free nation has ever achieved. A more recent and radical interpretation asserts that our schools have been custodial institutions, which

regiment children through a bureaucratic control completely unresponsive to the will of the public. The problem with the books presented here is not that they support one position or the other, but that they avoid coming to grips with this question. Schools may liberate and they may coerce; they may promote either cultural pluralism or homogeneity. It is unfortunate that these works carry us but a little way toward a better understanding of the manner in which the schools have made a difference in the development of our culture and the degree to which they shaped the personalities and enlarged the opportunities of the children attending them.

JONATHAN MESSERLI  
Fordham University

G. N. SEVOST'IANOV, editor, *Osnovnye problemy istorii SShA v amerikanskoi istoriografii: Ot kolonial'nogo perioda do grazhdanskoi voyny 1861–1865 gg.* [Basic Problems in the History of the U. S. A. in American Historiography: From the Colonial Period to the Civil War, 1861–1865.] (Akademiia Nauk SSSR, Institut Vseobshchei Istarii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 373.

This volume includes essays on American historiography of twelve major themes in United States history during the period before the Civil War. They range from the treatment of the Indians to the writings of Franklin and Paine, the influence of the frontier, and the abolitionist movement, and all essays show their compilers to have engaged in a surprisingly broad and diligent study of the relevant writings. One finds citations of material ranging from the earliest days to, approximately, the mid-1960s, and including not only such prominent historians as Bancroft, Turner, Beard, or Nevins, but also the more obscure chroniclers of the events of their own time, such as the New York clergyman A. McLeod, whose book of 1815 on the War of 1812 is cited to show something of then current opinion. It would be difficult for one reviewer to state with certainty that the expositions which these Soviet scholars make of the themes and interpretations of all the American historians referred to are always succinct and just, but there is evidence of a scholarly approach and of an appreciation of the complexities of American events and of their analysis. The influence of the Soviet world view is, of course, always evident, and this reviewer finds an implicit gradation of American historians against the scale of the degree to which they agree with Marxist and "progressive" points of view. Yet those who have some familiarity with other Soviet writings on American history can see that the analyses offered here are based on a wider knowledge of the literature and a greater feeling



for the nuances than has, at times, been the case previously. For example, M. V. Demikhovskii's study of the frontier and federal land policies manages, without citing references that Marx and Lenin made to the topic, to indicate that the Homestead Act may not have had so marked an effect as other Soviet historians, notably A. V. Efimov, have suggested.

There are, unfortunately, few American scholars whose knowledge of their own history is combined with a knowledge of the Russian language, so that this volume will undoubtedly find few readers in this country. This is disappointing, not so much because this volume provides any major new interpretation of American historiography, but rather because Soviet "Amerikanistika" deserves to be taken into account as an area of serious scholarship. Beyond the flow of ephemeral Soviet works dealing with the United States, there is a group of books and articles, particularly in history and literary studies, that is worthy of attention. Except for some studies in diplomatic history or comparative literature, these publications provide little in the way of new sources, but they often contain evidences of great diligence and of an effort to extend and refine Marxist interpretations of America. The present volume falls within this framework and, therefore, should be of interest to those who wish to see a very different point of view of American historiography.

ROBERT V. ALLEN  
*Washington, D.C.*

BENJAMIN FRANKLIN COOLING. *Symbol, Sword, and Shield: Defending Washington during the Civil War*. Hamden, Conn.: Archon Books. 1975. Pp. 300. \$12.50.

WILLIAM C. DAVIS. *The Battle of New Market*. Garden City: Doubleday and Company. 1975. Pp. xii, 249. \$8.95.

These two modest military histories, one dealing with a neglected and one a saturated subject, are well-composed, solid works which should become useful additions to the library of Civil War literature. Benjamin Franklin Cooling, Assistant Director for Historic Services at Carlisle Barracks, was fascinated by the wartime defense works around Washington as far back as childhood. Failing to find an adequate book on the subject, he has now written one himself. William C. Davis, editor of *Civil War Times Illustrated*, tackles a topic—the Battle of New Market—rich in lore and legend, but lacking a recent synthesis and evaluation.

Washington, "symbol" of the Union, was protected by the Army of the Potomac, the "sword," and a \$1,400,000 set of defense fortifications, the

"shield." At the time of First Bull Run the capital was virtually defenseless. The Confederate threat taught an important lesson. Forts were hastily thrown up around the perimeter of the District and across the Potomac in Virginia. In addition, the responsibility of safeguarding Washington would govern every future move of the Army of the Potomac. The cost was great, and although no serious challenge occurred, save for Early's raid, the author concludes that the investment to protect the capital was worth it. While a well-mounted Confederate force could possibly have broken into the city, the deterrent effect of the forts discouraged such a move.

Cooling has researched, organized, and written the story well, but the book suffers from the absence of readable maps. The innumerable diagrams of the various forts are of interest, but would be more useful if their positions were known. Readers must make do with one microscopic map of Greater Washington, which defies even a four-inch magnifying glass, and a sketchy layout of the Northern defenses at the time of Early's raid.

Maps are even more important in Davis' blow-by-blow account of the New Market affray of May 15, 1864, and he supplies them—good ones, too—in abundance. Moreover, he has read everything ever written about the battle and has squeezed the essence into this compact account. At times the compactness almost overpowers a reader not careful to keep up with all the regimental positions as they shifted about on that hectic day. But if you like your battles in minute-by-minute portions, you could ask for no more here.

Although Davis has provided us with a good up-to-date narrative of New Market, it is unlikely the student will discover a great deal he did not know before reading the book. Davis lays bare the shortcomings of the incompetent Franz Sigel, commanding general of the Federal forces. The fine generalship of John C. Breckinridge, the Confederate commander, emerges clearly, although one might wonder if Breckinridge was really "another Stonewall Jackson," if such a judgment is to be based on this one battle in which he faced an inferior foe. Davis notes, too, the heroism of the Virginia Military Institute cadets, but he is quick to point out how the legend of their noble effort has far surpassed the reality. There were, after all, 5000 or so other Confederates at New Market in addition to the 260 cadets. The Confederate victory at New Market certainly eased temporarily Lee's problems in Eastern Virginia. Still, the possibility remains that the struggle was not quite as significant as the author suggests.

E. C. MURDOCK  
*Marietta College*

HOWARD N. MEYER. *The Amendment That Refused to Die*. Radnor, Pa.: Chilton Book Company. 1973. Pp. xii, 230. \$7.95.

Despite its melodramatic title and the lack of notes or citations, this book by Howard Meyer is well worth reading and recommending for constitutional history classes. Meyer's book is more of a tale, filled with anecdotes and vignettes, than a work of legal or historical analysis. It is the story of "Big Fourteen," as the author calls the Fourteenth Amendment, from its inception in Madison's proposal in 1789 to extend key Bill of Rights provisions to the states, through its ratification in the late 1860s, on to its distortions in the late nineteenth and early twentieth centuries, and finally to its resurrection in the 1920s.

Often simplistic and seemingly oblivious to legal and historical controversy (progressive historiography is still alive and well with non-professionals), Meyer's book is nevertheless solidly based on the legislative and judicial history of the Fourteenth Amendment. This reviewer is convinced that Meyer is right in the long, bitter debate over whether the Fourteenth Amendment was intended to include the Bill of Rights. The Fourteenth Amendment did indeed contemplate a national and unitary citizenship for all Americans; and the rights of that citizenship surely included the principles of the Bill of Rights.

Meyer's review of the twentieth-century history of the Fourteenth Amendment is more spotty but includes short chapters on a wide range of topics—the rise of the NAACP, the women's suffrage movement, the dramatic civil liberties cases of the 1920s, and the judicial breakthroughs of the 1940s and 1950s. The emphasis is biographical and episodic, touching on dozens of distinguished personalities who contributed to regeneration of the Fourteenth Amendment.

Doubtless contemporary psychohistorians and behaviorist quantifiers will smile at much of Meyer's presentation. But it is salutary that there continues to be some old-fashioned, "good-guy" history, especially in so forbidding a field as Fourteenth Amendment historiography. Meyer will hardly replace Howard J. Graham or Edward S. Corwin on the Fourteenth, but the epic chronicle still has its role in the history of a people and their institutions.

ARNOLD MILTON PAUL  
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JOHN Y. SIMON, edited, with notes and foreword by. *The Personal Memoirs of Julia Dent Grant [Mrs. Ulysses S. Grant]*. With introduction by BRUCE CAT-

TON. With a foreword by RALPH G. NEWMAN. New York: G. P. Putnam's Sons. 1975. Pp. 346. \$12.50.

PAUL E. FULLER. *Laura Clay and the Woman's Rights Movement*. Lexington: University Press of Kentucky. 1975. Pp. ix, 216. \$12.50.

The two women who furnish the subject matter for these two books played two entirely different roles. Julia Dent Grant was the wife of Ulysses S. Grant, and very conscious of this fact. Throughout the book she refers to herself by her husband's titles, and she refers to other women in the same way. In fact, sometimes only her use of a new title indicates that her husband has been promoted or changed jobs. Laura Clay was a feminist, and after 1888 she became a power in the suffrage movement, first in her home state of Kentucky, then later on the national scene. Both books probably owe their publication to the current concern about women.

Mrs. General Grant, as she often called herself, wrote her memoirs after the death of her husband in 1885, partly to supplement his own memoirs, partly to defend her husband against any imagined slight, and partly, if not mainly, for money. One of the reasons the memoirs did not appear in her own lifetime was that no publisher would meet her price; ultimately reconciled to this she tried to sell the manuscript to Andrew Carnegie for \$125,000. She rarely discusses issues in the world at large, perhaps because she regarded her work as supplementing that of her husband. There is little evidence in the memoirs that she understood most of the issues. She is certain that her Ulysses was a good man; she goes to great effort to clear up any alleged drinking episodes and deny that her husband had any part in the scandals which afflicted the White House. Much of her concern is to tell us what she wore, where she sat, what she ate, and whom she met. Occasionally she offers some interesting sidelights, but even with the Lincoln assassination she concentrates on her husband. A large part of the book is devoted to the trip around the world that she and the general undertook after they left the White House. Julia Dent Grant was a devoted and proud wife, as well as a concerned mother. The book is ably edited by John Y. Simon, who identifies most of the individuals and incidents mentioned. Its value is for the insights into Julia's personality; and inasmuch as Julia reflects other women of the time, the book offers a glimpse into the limited lives of the upper-middle-class women married to the powerful and famous.

Laura Clay is a different kind of person, although her father, somewhat unsuccessful as a businessman or farmer, had served as Lincoln's

minister to Russia. The Clay family stayed only a few months in Russia, however, before her mother, Mary Jane Clay, left her husband and returned to Kentucky with her family. Here Mary Jane took over a nearly bankrupt family farm and made it extremely successful. After the return of her husband there was an attempt to reunite the family, but this failed because of the philandering of Clay. Mary Jane sued successfully for divorce, but in the process lost the benefits of most of her life's work. This inequity imposed upon their mother apparently impressed the Clay women, for all entered into the suffrage movement, although only Laura, the spinster, devoted her life to it. She was able to *do so because of an inheritance from her father*, part of the family farm that her mother had built into a thriving enterprise. One of the main themes in Paul Fuller's able account of the suffrage movement is the importance that money played in its leadership. Laura Clay, in part because she controlled some funds, loomed large on the national scene, and she was supplanted only when others who had more money or access to more money emerged.

Clay's career with the women's movement was not without controversy, and Fuller devotes considerable space to recounting Clay's break with the movement over the decision of its leadership to go full speed ahead for the Nineteenth Amendment. Throughout her career Clay had adhered to a states' rights position, and she did not change easily. Fuller attempts to explain Clay's backing of a literacy test combined with women's suffrage in the Southern states as a realistic approach instead of playing to the inherent racism of the region. His argument fails to convince, although he makes it clear that Clay was no bigot; this was evidenced by her campaign for the election of Al Smith in 1928, despite her long-time support for the WCTU. Her support of the WCTU, and perhaps her support of literacy tests in the South, can be explained in part by a willingness to face political reality. It is obvious that she was very much a woman of her time and region. She was also a politician who believed that women's rights were the most important issue facing the country. Although she had strong principles, the goal that counted most was that of women's rights, and she fought for this in the Church, in the Democratic party, and on the national scene.

The author has used the Laura Clay papers at the University of Kentucky, a particularly valuable collection since most of the other leaders destroyed their own papers. Laura Clay's career presents the other side of woman's life and a clear contrast to the experience of Mrs. General Grant. For her role Laura Clay had to sacrifice marriage and family,

the very things that Julia Dent Grant held most dear.

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KRISTIAN HVIDT. *Flight to America: The Social Background of 300,000 Danish Emigrants*. (Studies in Social Discontinuity.) New York: Academic Press. 1975. Pp. viii, 214. \$13.50.

H. ARNOLD BARTON. *Letters from the Promised Land: Swedes in America, 1840-1914*. Minneapolis: University of Minnesota Press, for the Swedish Pioneer Historical Society. 1975. Pp. vi, 344. \$16.50.

DOROTHY BURTON SKÅRDAL. *The Divided Heart: Scandinavian Immigrant Experience through Literary Sources*. With a preface by OSCAR HANDLIN. Lincoln: University of Nebraska Press. 1974. Pp. 393. \$20.00.

EMORY LINDQUIST. *An Immigrant's American Odyssey: A Biography of Ernst Skarstedt*. (Augustana Historical Society, number 24.) Rock Island, Ill.: the Society. 1974. Pp. xi, 240. \$5.95.

Each of the books under consideration contributes to a broader and clearer view of Scandinavian emigration and of Scandinavian-American life.

Kristian Hvidt's *Flight to America* is a welcome response to a long-felt need for a scholarly interpretation of Danish migration to the United States in the nineteenth and twentieth centuries. With a remarkable economy of words, he views this migration from 1868 to 1914 against a background of European and Atlantic forces and compares it with that of other countries, especially Norway and Sweden. His major source is a collection of emigration records kept by police officials, mostly in Copenhagen, and he deals statistically with persons who left Denmark before 1900, after which time the records become somewhat unreliable. After analyzing his data with the aid of a computer, Hvidt proceeds with a search for the "why." Although his answers are similar to those of scholars who have studied the migrations from Norway and Sweden, he makes clear that 44 percent of Danish emigrants before 1900 were from towns or cities and that few land owners joined them. His explanation is found in the relatively early industrialization of Denmark and in geographical conditions that had made internal migration easier than in Sweden and Norway. This industrialization was not sufficient, however, to absorb easily the increase in population; as a consequence, many people left either for Copenhagen or America, or both. The Danes who emigrated were those who had the greatest difficulty in adjusting to a chang-

ing society—craftsmen who were the immediate victims of industrialization, wage earners who heard of better opportunities in the New World, and rural workers unable to acquire land holdings before 1899.

Hvidt recognizes the importance of popular education in promoting emigration and emphasizes the role played by personal relationships across the Atlantic. His treatment of settlement in the United States and of Danish-American life, however, is sketchy in the extreme. Unfortunately, this English translation of his original *Flugten til Amerika* does not contain his brilliant analysis of the activity of steamship lines, railroads, industry, and government agencies—the “intermediaries and the connecting links between the push and pull factors” in migration.

Although some “America letters” written by Swedish immigrants have been translated, edited, and published in periodicals, H. Arnold Barton is the first scholar to bring a substantial number of these together in a single volume. His *Letters from the Promised Land* thus provides students of immigration with a collection of valuable sources. He has drawn some materials from accounts of Swedish travelers, reminiscences, and diaries, but in the main the documents he uses are letters written by persons, often of slight education, actually experiencing the hardships, frustrations, successes, and excitement of immigrant life. Barton has not hesitated to edit the letters liberally, but he has nevertheless succeeded in retaining their original flavor. In addition, he offers a short introduction, an excellent background essay for each of the three parts into which the documents are divided, a helpful epilogue, and a select bibliography. His footnoting is both skillful and useful to the person unfamiliar with Swedish terminology and institutions.

As Barton’s subtitle, *Swedes in America, 1840-1914*, suggests, he has attempted to cover, in time, the whole of Swedish immigration in the nineteenth and twentieth centuries; he has also applied the same broad measurements to space, even reaching out to Hawaii. In part one he deals with the pioneers in the Middle West, South, Far West, and East to 1864, then turns to what he calls the “Great Farmer-Land in the West,” a story he carries to 1889 in part two, and covers in part three “Farm, Forest, and Factory” from 1890 to 1914. Barton has attempted to produce a general review of Swedish migration with sources and short interpretive sketches, and has been influenced as much by reader interest as by a need to document major episodes in the Swedish story. He has succeeded remarkably well, and the Swedish Pioneer Historical Society deserves praise for publishing his book.

In *The Divided Heart*, Dorothy Burton Skårdal attempts to interpret the painful assimilation of Scandinavian immigrants into American life through the imaginative writings of Danish, Swedish, and Norwegian immigrant authors. Although the almost countless writers she includes tend to become lost in notes at the rear of the book, their comments on a variety of themes—the European background, the Atlantic crossing, cultural adjustments and conservation, and American economic, social, educational, religious, political, and other practices and institutions—are quoted at length in the text.

Skårdal is particularly interested in the changes wrought by American materialism on old-country values and in the changes that took place in the New World in immigrant morals and mores, and she gives considerable attention to the problem of success in the New World and the pressures it brought to bear against the European heritage. In the end she describes the “pain of a divided heart” and draws up a balance sheet of loss and gain for the immigrant. She forcefully urges the importance of creative literature as a source for the study of Scandinavian-American life; few historians will now dare to ignore it wholly in favor of the more matter-of-fact remarks found in “American letters” and similar records. It would be folly, however, to assume that migration can be understood solely from the imaginative works of sensitive poets and novelists, even when they are in agreement. One may ask, too, if a volume focusing on the personal sufferings, frustrations, and creative works of a gifted group of transplanted writers would not have contributed even more to an understanding of the complex problem of assimilation and the subject of surviving ethnic feelings among the “old immigrants.”

Emory Lindquist presents a careful study of Ernst Skarstedt, one of the authors discussed briefly by Skårdal. Lindquist titles his biography *An Immigrant’s American Odyssey*, an apt term for the American career of the Swedish vagabond-poet-editor-farmer-champion of freedom. Skarstedt may not have been a top-flight poet, but his observations of the Swedish immigrant, especially in *Svensk-amerikanska folket i helg och söcken*, are both detailed and penetrating. Son of a well-known professor of theology in the University of Lund, he took to the sea early in life, scorned his European class advantages, and came to the New World in 1878. Here he lived much of his life as something of a hermit in the Puget Sound region, writing constantly and leaving his farm from time to time to work on Swedish-language newspapers in Chicago, New York, and San Francisco. Fortunately, Skarstedt kept detailed diaries from early years



and Lindquist makes good use of them as well as the published writings of the poet. What emerges from the biography is something much more than an eccentric rebel and cultural conservationist—a gifted individualist who decries the superficial trappings of American life, but champions the basic ideals of his adopted country and, surprisingly, defends the performance of the captains of industry in an age of exploitation.

All four books are well edited, contain adequate bibliographies and indexes, and—with the exception of *The Divided Heart*—are illustrated. More significantly, their authors also accept the presupposition of the progressive school of historians: complete assimilation of the immigrant into an improving American society. The common theme therefore is transition. Together, they are a valuable addition to scholarship in a long-neglected but vital phase of American history.

KENNETH O. BJORK  
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RICHARD E. LINGENFELTER. *The Hardrock Miners: A History of the Mining Labor Movement in the American West, 1863-1893*. Berkeley and Los Angeles: University of California Press. 1974. Pp. viii, 278. \$12.50.

The subtitle of Richard Lingenfelter's monograph accurately defines the scope of this chronological and institutional history of the hardrock miners' unions. The introductory chapter characterizes the miners and their environment; the remaining chapters trace the course of these industrial unions from their inception in Virginia City to the formation of the Western Federation of Miners in 1893. The book complements the existing literature by describing the fitful extension of unionism from the 1860s to the early 1890s. Though miners lacked a regional organization before the WFM, individuals transmitted the union idea, as evidenced by the widespread adoption of local constitutions modeled upon those drafted on the Comstock and in Butte.

Using newspapers, periodicals, and existing union records, Lingenfelter examines stands on the controversial issues of wages, hours, technological innovations (for example, dynamite), and Chinese laborers. He argues that miners' organizations were a direct response to impersonalization stemming from "growing industrialization, capitalization, and absentee ownership of the western mines" (p. 30). He views the organizations as but another aspect of the national reaction to the extension of industrial capitalism.

The study does not define precisely how the term "industrialization" is applied to hardrock mining. Focusing upon the unions and the capital-

ists, Lingenfelter gives scant attention to the sentiments and reactions of resident owners of small properties or of unaffiliated mine laborers. He shows clearly, however, the link between declining mineral prices and management pressure for wage reductions. In the 1890s this situation became the fateful catalyst for the Coeur d'Alene labor war.

Though strikes and crises predominate, this work does cast additional light upon routine union activities and thereby enriches our understanding of miners and their concerns. Carefully researched and well written, it makes an important contribution to the history of both the American labor movement and the American West.

RONALD C. BROWN  
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D. C. BLOOMER. *Life and Writings of Amelia Bloomer*. With a new introduction by SUSAN J. KLEINBERG. (Studies in the Life of Women.) Reprinted from the Arena Press edition of 1895. New York: Schocken Books. 1975. Pp. xv, 387. Cloth \$7.50, paper \$3.95.

ELIZABETH EVANS. *Weathering the Storm: Women of the American Revolution*. New York: Charles Scribner's Sons. 1975. Pp. 372. \$12.50.

MARY P. RYAN. *Womanhood in America: From Colonial Times to the Present*. New York: New Viewpoints. 1975. Pp. 496. \$15.00.

While historians of the United States have become increasingly conscious of the need to integrate the history of women into their teaching curriculum, the lack of publications appropriate for classroom use has hampered such integration. These three books offer the history teacher a means for transcending this pedagogic barrier. Ryan's text covers the entire sweep of the history of American women, Bloomer's writings focus on the pivotal shift to overt feminism in the mid-nineteenth century, and Evans' anthology of diaries and journals from the Revolution makes heretofore obscure primary sources easily accessible.

Mary Ryan's *Womanhood in America* is a concise and scholarly text that should remain authoritative for many years to come. Its merits are those characteristic of all good history and few textbooks—patience with the complexities of continuity and change, effective presentation of primary evidence, and a persuasive interpretation of secular trends. Necessarily highly selective, the book emphasizes three central themes of female experience: family, work, and feminism. The first four of its seven lengthy chapters consider pre-twentieth-century developments: "Women in Agrarian Society," primarily before 1750; "Women in Commercial America," primarily from 1750 to 1830; "The Common Woman, 1830-1860"; and "Work-



ers, Immigrants, Social Housekeepers: Women and the Industrial Machine, 1860-1920." The last three chapters focus on more recent history: "The Sexy Saleslady: Psychology and Consumption in the Twentieth Century"; "A Kaleidoscope of Roles: Twentieth-Century Women at Work and in the Home"; and "Current Snares of Sexism: Inequality in Any Class."

Ryan's footnotes reflect recent social history as well as rich primary material, and her index is a guide to the main issues and contours of the history of American women. Aware of the importance of class, ethnicity, race, and region, she is careful to draw distinctions among different groups of women, but also clarifies the historical circumstances women have shared because of their gender. The demographic, social, and intellectual conditions of motherhood among various groups of women and the economic circumstances shaping their patterns of work receive special attention as do the changes of consciousness that promote feminism. Ryan's discussions of changes in sexual behavior, women's legal status, and female stereotyping make her book as comprehensive as it is insightful.

In a more specialized vein, but equally valuable for classroom use, is D. C. Bloomer's *Life and Writings of Amelia Bloomer*, with an interpretive and scholarly introduction by Susan J. Kleinberg. Bloomer (1818-1894) both reflected and symbolized the generation of mid-nineteenth-century, middle-class women whose participation in social reform drew them into the assertion of autonomous female politics and activism. Marrying an anti-slavery reformer and Whig editor of the *Seneca County Courier* in 1840 (the word "obey" being omitted from the bride's vows), Amelia Jenks Bloomer settled in Seneca Falls, where in 1848 she attended Elizabeth Cady Stanton's famous convention on women's rights and in 1849 founded, with other Seneca Falls women, a "little temperance paper," the *Lily*. Gradually assuming editorial leadership, Bloomer advocated women's rights as well as temperance. In her writings, students will discover the ferment of the early women's movement as well as its connections with other reform movements.

Dress reform and the *Lily*'s advocacy of Turkish pantaloons was but one aspect of the early feminist effort to free women from the rigid conventions surrounding their behavior. Eventually dropped because Bloomer, Stanton, and others concluded that "dress was drawing attention from what we thought of far greater importance," dress reform gave way to questions of "woman's right to better education, a wider field of employment, to better remuneration for her labor, and to the ballot for the protection of her rights." The *Lily* attained a

national circulation of over 6,000 by 1853, but it ceased publication in 1856 after the Bloomers moved west to Council Bluffs, Iowa. In 1869 Amelia Bloomer represented Iowa at the meeting of the American Equal Rights Association in New York City, and in 1871 became president of the Iowa Woman Suffrage Society. An effective public speaker, writer, and editor of one of the first women's rights newspapers, Mrs. Bloomer epitomizes the westward movement of women's rights as well as its Seneca Falls origins.

While clearly conceived to celebrate the Bicentennial and collected by a journalist rather than a historian, the eleven diaries and journals contained in *Weathering the Storm* constitute a valuable classroom tool. Elizabeth Evans has provided a descriptive introduction to each journal that places it in its immediate historical context. They range from the flirtatious Sarah Wister, who describes her youthful attraction to soldiers garrisoned near her Pennsylvania home, to the avid patriotism of Deborah Sampson Gannett, a former indentured servant who dressed as a man and fought for three years in the Fourth Massachusetts Regiment; from Grace Growdon Galloway, who records her determined but unsuccessful fight to retain her property when that of her loyalist husband was confiscated, to Margaret Hill Morris, a young Philadelphia widow, Quaker, and neutralist, who reports on the battle of Trenton and the difficulties of raising four small children in wartime. The accounts of these and other women in the Revolutionary era make their war experiences vivid and meaningful for today's students.

KATHRYN KISH SKLAR  
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GEORGE M. WOYTANOWITZ. *University Extension: The Early Years in the United States, 1885-1915*. (Series on Continuing Education, number 2.) Iowa City: National University Extension Association and the American College Testing Program. 1974. Pp. xi, 171. \$3.00.

AMERICO D. LAPATI. *Education and the Federal Government: A Historical Record*. New York: Mason/Charter. 1975. Pp. v, 388. \$15.00.

"Soap and education are not as sudden as a massacre," wrote Mark Twain at the turn of the century, "but they are more deadly in the long run." The quests for health and learning are inherent elements in the American story, the latter of which is reflected in the material contained in these two books. Both are commendable additions to the bibliography of the history of American education.

In a well-written, well-documented monograph George M. Woytanowitz traces the first thirty years of university extension programs in the

United States. Beginning with the background of English university extension, he then proceeds to discuss, in turn, the background of American university extension (the lyceum, Chautauqua, and Johns Hopkins University); extension in the 1880s (noting that "Herbert Baxter Adams, professor of history at Hopkins, was the first to push the idea of university extension in America," [p. 24]); the founding and activity of the American Society for the Extension of University Teaching and the first national conference on university extension; extension in the West through the efforts of the Universities of Chicago, Kansas, and Wisconsin; the "seven 'at years: 1892-1899"; the impact of university extension on American education; the relationship of university extension to the urban crisis; and the decline and reorientation of extension between 1900 and 1915.

The author provides a careful assessment of reasons why in the late nineteenth century "of all the educational innovations, university extension was the least successful in winning acceptance from either educational leaders or the interested public" (p. x). He shows how after 1900 university extension underwent a transition, becoming "more entertaining than educational."

Although *University Extension* includes an extensive, though selected bibliography, it unfortunately contains no index, a "must" for a study wherein many names of individuals, institutions, and organizations are mentioned.

In *Education and the Federal Government* Americo D. Lapati provides a valuable reference work. Divided into two parts, "Congress and Education" and "The Supreme Court and Education," this volume begins with a brief history of the Office of Education. In chapters two through five the author discusses seriatim the many acts and programs related to education enacted by Congress, beginning with the Ordinances of 1785 and 1787. For example, in the chapter on higher education, Lapati takes up plans for a national university, the service academies, Howard University, the Morrill Acts, the National Youth Administration, the G.I. Bill of Rights, the Higher Education Facilities Act of 1963, and prohibition of sex discrimination (Title IX). Other chapters in Part I relate to federal legislation affecting elementary and secondary education, vocational education, rehabilitation, and the handicapped, as well as international education.

Part II begins with a chapter devoted to court decisions concerning the financing and control of public education. The remaining chapters deal with decisions of the Supreme Court regarding private and religious schools; religious teaching and activities in the public schools; teachers (for example, contracts of employment and tenure, loy-

alty oaths, and freedom of expression and association); students (for example, secret societies in schools, conscience—military training and compulsory flag saluting—and corporal punishment); and integration.

The author provides ample documentation and a selected bibliography for this important facet of American history, and he presents the material in clear, concise prose that deserves approbation.

WILLIAM LLOYD FOX  
Montgomery College

CLAY MCSHANE. *Technology and Reform: Street Railways and the Growth of Milwaukee, 1887-1900*. Madison: the Society Press of the State Historical Society of Wisconsin, for the Department of History, University of Wisconsin. 1975. Pp. ix, 187. \$5.95.

During the last decade of the nineteenth century electric street railways were replacing horse-car routes in hundreds of American cities. Larger cars, faster service, and extended routes to suburban areas all resulted from the change in urban transportation first introduced by Charles Van Depoele and Frank Sprague. Clay McShane's study carefully reviews the experience of Milwaukee as that city adopted and sought to regulate this important new technology.

The economy of Milwaukee late in the nineteenth century was shifting from a major dependency upon flour milling toward an industrial mix of breweries, meatpacking plants, and factories. The new plants, several of which were located outside the city limits, employed much of the large German- and Polish-born population of the city. Growing from a population of 115,000 in 1880 to 285,000 in 1900, Milwaukee was ready for innovation in urban transportation. Henry Clay Payne, Milwaukee businessman and a prominent Wisconsin Republican politician, played a vital role in merging several horse-car lines into a consolidated street railway system. Possessing both a favorable municipal franchise and financial backing from Henry Villard of the Northern Pacific, Payne was able by 1893 to fully electrify his new Milwaukee Street Railway Company. The depression of 1893 ended the real estate boom in Milwaukee, and a decline in trolley traffic forced the traction company into bankruptcy. Payne managed successfully to reorganize his company and was also victorious over civic reformers in disputes concerning taxes, lower fares, and route extensions. His triumph over the reformers was complete with the enactment of a new franchise approved in 1900.

The author reviews early electric traction in Milwaukee with clarity and a full appreciation for the

varied economic and political forces involved. Perhaps his introduction to the subject is too extensive, since four of the eight chapters cover mainly background material. *Technology and Reform* is a valuable addition, however, to the growing literature on electric traction in urban America.

JOHN F. STOVER  
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JOSEF J. BARTON. *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City, 1890-1950*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1975. Pp. xii, 217. \$12.00.

A regrettable cleavage has developed for about a decade now between two groups of American social historians who ought to be more compatible, those of the "new urban" history and immigration specialists. The division has persisted not only because of topic but also because of their differing emphases on methodology. Urbanists have heralded quantification, while most "ethnicists" have stressed the more traditional but heretofore neglected group materials. Owing to the difficulty in mastering both techniques, few substantial studies, with the possible exception of demographic or political histories such as Frederick Luebke's work on Nebraska Germans, have satisfactorily integrated both the newer and older methods. Josef Barton, more advantageously than in any previous work, employed both and thereby has contributed to the restoration of a necessary dialogue. This lucid account of the social determinants of Italian, Slovak, and Rumanian ethnic adjustment in an American city has established a new standard of scholarship for historians of urban and ethnic life. In its methodological synthesis, the work inaugurates a new generation of younger ethnic historians.

Barton demonstrates an additional talent in employing still another new approach, the comparative, cross-cultural technique. Immigration and ethnic historians are well aware of the research difficulties in studying individual groups other than their own, language being only one of several handicaps. While Barton had the help of the rich immigrant collection at the University of Minnesota, he shows a remarkable individual sophistication in comparing three dissimilar ethnic groups, especially at their place of origin.

His discussion of emigration conditions in Eastern Southern Europe offers no surprises to the knowledgeable immigration specialist. The motivations, social structure, and traveling patterns of the departing waves, for economic and status reasons via pioneer, chain, and family migration, are

familiar to readers of Balch, MacDonald, Banfield, Vecoli, and others. Nowhere to this reviewer's knowledge have these insights been comparatively synthesized and, more important, linked to the groups' social mobility in America. One revealing and provocative conclusion, for example, is the close correlation between the immigrant father's mobility and that of his child; variables of education or family size are less significant.

It is in relating his place and subjects to a larger ethnic and urban framework that Barton is weakest. While effectively employing the several methodologies, his work also suffers from their defects. He focuses excessively, for example, on one city—Cleveland—with the implied assumption that it is a typical laboratory for late nineteenth-century ethnic adjustment. The reader moreover gains an insufficient sense of the relationship of Cleveland's Slovaks, Rumanians, and Italians to their American communities in general. Ethnic historians would seriously question Barton's suggestion that his three groups shed much light on other "similar" peoples—Poles, Hungarians, and Greeks. The latter are simply too (though not entirely) dissimilar in composition.

These limitations, in dealing inadequately with broader segments of America's past, are as much a weakness of the urban and ethnic fields as of Barton's work per se. We will have to await later studies by him and his colleagues. He has produced a work of distinguished scholarship, and this reviewer hopes he eschews his intended next study and remains essentially an Americanist.

VICTOR GREENE  
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STUART GALISHOFF. *Safeguarding the Public Health: Newark, 1895-1918*. Westport, Conn.: Greenwood Press. 1975. Pp. xv, 191. \$13.50.

This narrative account of the development of public health in Newark is derived from a variety of sources whose unearthing and mastery were no mean feats. In addition to the few pertinent manuscript sources, the author has based his work primarily on newspapers and ephemeral reports and notices of relatively obscure public, quasi-public, and private agencies.

Beginning with an introduction, "The Nation's Unhealthiest City," as the census dubbed Newark in 1890, the author traces the development of the Newark Board of Health and the bacteriology laboratory and the problems and control of the water supply, of sewage, and of mosquitoes. He then describes the movement for "Milk Reform," in which Henry L. Coit and his introduction of certified milk gave Newark a prominence not matched

by the city's actual accomplishment. Galishoff also covers the campaigns against infant mortality, tuberculosis, and industrial disease, and the problem of tenement housing. Pertinent morbidity and mortality data and other statistics are included.

Although Newark's accomplishments, especially with regard to infant mortality and tuberculosis, were considerable, the work claims no extraordinary feats for the city. The author rightly warns against generalizing on the basis of Newark's experience, but he does offer a not atypical case history of the political, economic, social, and personal forces that sometimes fostered, sometimes hindered, progress in public health.

The time limit set by the author is artificial, but it does reflect an attempt to associate the public health improvements with the Progressive era. Only in the conclusion of the book is there an attempt to make this connection. Although the author may be correct in his contention that in seizing upon the "revolutionary discoveries made in sanitary engineering and medical science, the Progressives effected a profound reduction in morbidity and mortality," his study does not effectively prove it. It is difficult to think of engineers and physicians—medical societies opposed the distribution of diphtheria antitoxin by public dispensaries—as Progressive reformers. The humanitarian instincts that motivated physicians like Henry L. Coit and Julius Levy (who was largely responsible for a successful attack on the high infant-mortality rate) may not reflect reformist social or political attitudes. In any event, the author gives no indication that either was in any way aware of or involved in Progressivism. There are similar difficulties with the many voluntary organizations that forced the problems of public health out of the private and into the public sector: nothing in the book demonstrates a relationship between their social consciousness and humanitarianism on the one hand and progressive social and political philosophy on the other. Only the newspapers, especially in their campaigns for pure milk, clearly fell into the muckraking spirit of the Progressive era. The public health movement may have reflected or benefited from the Progressive movement, but the author has not come to grips with the question.

An occasional error in footnoting is discernible—a wrong date, a wrong page, a misplaced attribution, an entirely wrong attribution—but the documentation is thorough. At only one point was significant documentation lacking: the author refers to the patronizing of immigrants by physicians and observes that "Levy was proposing to teach 'dumb' immigrants how to guard their infants against serious disease" (p. 112). It would be sig-

nificant to know whether Levy, a dedicated humanitarian, actually used the epithet.

DAVID L. COWEN  
Rutgers University

WILLARD B. GATEWOOD, JR. *Black Americans and the White Man's Burden, 1898-1903*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. xi, 352. \$12.95.

Several studies of various aspects of American imperialism at the turn of the twentieth century have appeared recently, but this admirable volume covers an aspect of the issue insufficiently developed in other works. The book's considerable strength lies in its analysis of articles in black newspapers, letters of black soldiers, and other materials of the period to show how blacks dealt with the issue of imperialist expansion. The author probes the basic dilemma facing blacks as they confronted this issue and argues convincingly that they viewed it differently from white citizens. As members of a racial minority all too often subjected to discrimination and violence in their own country, Negro Americans quickly identified with the struggle for national independence by the oppressed populations of Hawaii, Cuba, and the Philippines. Yet they had to take care lest their support be used to advance the purposes of those who wished to annex these territories as part of the new American empire. To be sure, there were black Americans who favored such acquisition, notably Edward E. Cooper, editor of the *Colored American*, a weekly published in Washington, D.C. But as Gatewood demonstrates, blacks favorable to acquisition were a distinct minority, and, moreover, were linked to Republican policies through political appointments.

Black participation in the war against Spain was favored by a majority of the black press on the ground that it would improve the status of the Negro people in the United States. But as Gatewood makes clear, it had the opposite effect and was followed by an intensification of racist violence. It is perhaps unfortunate that this discussion of the relationship between imperialism and racism does not include recognition of the belated understanding on the part of important white anti-imperialists that imperialism had to be combatted precisely because it was intensifying racism in this country.

Those already familiar with Gatewood's collection of papers on black American soldiers in the war against Spain and his volume of letters by Negro soldiers covering the years 1898-1902 will not be surprised by his perceptive use of sources and felicitous writing. In the present work, he has



fulfilled the expectations aroused by his earlier writings. He has produced a scholarly and readable book for which all who work in the era of the "nadir" of black American history will be grateful.

PHILIP S. FONER  
Lincoln University

JULES A. KARLIN. *Joseph M. Dixon of Montana. Part 1, Senator and Bull Moose Manager, 1867-1917; Part 2, Governor Versus the Anaconda, 1917-1934.* Missoula: University of Montana. 1974. Pp. xiii, 257; xvii, 269.

For a sparsely populated state Montana has produced a surprising number of nationally prominent political figures: Jeannette Rankin, Thomas J. Walsh, Burton K. Wheeler, James E. Murray, and Mike Mansfield. Jules A. Karlin, professor of history at the University of Montana, believes Joseph M. Dixon belongs on the list. Born in North Carolina and reared by Quaker parents, Dixon was twenty-four years old when he arrived in Missoula in 1891. After admission to the bar, Dixon embarked upon a long political career, successively as Missoula County Attorney, state legislator, congressman, senator, governor, and Assistant Secretary of the Interior in Herbert Hoover's administration. During his first years in Washington (1903-1913) he became a friend of Theodore Roosevelt and espoused some politically reformist ideas.

These volumes, the product of more than twenty years of effort, are based on Dixon's papers which the author supports with an impressive list of other manuscript sources, interviews, and about 150 newspapers. To conserve space, he used few footnotes, so anyone wishing to use these voluminous sources will have to turn to the original manuscripts in the University of Montana archives.

Although he managed Theodore Roosevelt's 1912 pre-convention campaign for the presidential nomination and was later chairman of the Progressive party's national committee, Dixon was a minor figure in American political history. He was a more important force in the history of Montana. These volumes will be of interest to local and regional historians and to students of state and local politics. Their chief value lies in the massive amount of details Karlin has assembled and the leads they will provide to future local historians. A microscopic study of a career such as Dixon's might have shed light on our understanding of progressivism by testing some of the ideas historians have propounded in recent decades. Unfortunately, Karlin's books are so badly lacking in conceptualization they confuse rather than clarify. The reader is not even clear what kind of progressive Joseph M. Dixon was. The only clear point is that in Karlin's mind Dixon was just about the

only public figure in Montana who supported progressive reforms, at least honestly so, and all who were not pro-Dixon were controlled by Anaconda, Montana Power, and their other corporate allies. The closest the reader gets to any sort of analysis is to be told over and over again that Dixon (whom the author compares to David) was pitted in what was ultimately a losing struggle against the Goliath of Anaconda's corporate power.

Karlin's prose style is annoying. The volumes abound with archaisms—"erstwhile" is worked to death. A confusing array of minor characters are identified by meaningless parenthetical descriptions which do not establish their importance to the story. But worst of all is the author's pomposity: "Dixon's inhalations of the election-scented air of 1914 did not affect him to the same degree as previous exposures to similar aromas" (Part I, p. 204).

Withal, these paperbacks are physically ugly. The first volume in particular is marred by many typographical errors. It also broke apart on its first use. The second is held together by huge staples, which means that it cannot be laid open, and holding it is like clutching a coiled spring. The uneven right-hand margins help create the impression of a badly typed thesis.

Finally, the books will fail in their main purpose. Karlin's uncritical hero worship will not rescue Dixon from obscurity.

RICHARD B. ROEDER  
Montana State University

JAMES B. LANE. *Jacob A. Riis and the American City.* (National University Publications, Interdisciplinary Urban Studies.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. xi, 267. \$12.50.

ALLEN F. DAVIS and MARK H. HALLER, editors. *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790-1940.* Philadelphia: Temple University Press. 1973. Pp. ix, 301. Cloth \$9.95, paper \$3.25.

FRANCIS RUSSELL. *A City in Terror: 1919—The Boston Police Strike.* New York: Viking Press. 1975. Pp. v, 256. \$10.00.

James B. Lane's *Jacob A. Riis and the American City* is a scholarly biography of Riis and his role as a reformer. The book is an outgrowth of a doctoral thesis, and unfortunately it reads too much like one. Yet Lane has carefully researched his subject and has written a thorough study.

Lane concentrates on Riis' attitudes toward the city, but he also devotes a considerable amount of time to the relationship between Riis and Theodore Roosevelt. The Riis that emerges in Lane's account is a middle-class Progressive. Although



Riis had an optimistic view of the world and was concerned about poverty, immigrants, and cities, he shared many of the Progressive attitudes about poverty and reform. He also worshipped Roosevelt and was uncritical about American foreign policy; the immigrant Riis became an American patriot. Lane is aware of the limitations of Progressivism, but nonetheless believes that Riis played an important part in alerting the American public to the problems of the city and the poor. There is no doubt that he did; Riis was one of the first to catch the national eye about these problems and he was an important minor figure of Progressivism. In spite of his relationship to Roosevelt, Riis appears to have been less influential after 1900. The student of reform will not learn much new about Progressivism, but rather will gain an understanding of one reformer.

Most of the essays in *The Peoples of Philadelphia: A History of Ethnic Groups and Lower-Class Life, 1790-1940*, are original. They are uneven in quality, but on the whole make rewarding reading for the student of urban and ethnic history.

Bruce Laurie's excellent article on fire companies and gangs demonstrates the use of material on fire companies in studying ethnic and working-class life. Hershberg also has some interesting things to say about the impact of urbanization on black life. The articles by Dennis Clark on the Irish, Caroline Golab on the Poles, Italians, and Jews, and Richard Varbero on the Italians are informative and offer the possibility of comparisons between Philadelphia's ethnic minorities and those of other cities.

Mark Haller suggests in the conclusion that the two overriding themes of these essays are the relationship of social change to crime and violence in the city and the interaction between the values of newcomers and the institutions of the city. Actually the essays do not dwell much on crime and violence, except for the mid-nineteenth century. This reviewer did not always find the issue of adjustment of values to the city pronounced in the essays. One should not be disturbed by a lack of unity in the articles; for, as the editors note, we know so little about lower-class and ethnic life in Philadelphia that the volume is useful as a beginning. The reviewer hopes that these scholars and others will continue their probing of Philadelphia.

Unlike the authors cited above, Francis Russell has written a popular history. *A City in Terror* is a compelling and lively book about the Boston Police Strike of 1919. He omits footnotes and has only a limited bibliography. For example, under Public Sources and Official Reports he ends his sketchy list by noting the "annual reports of the police commissioner, Harvard class reports, and so forth."

*A City in Terror* has plenty of drama and heroes and villains. Russell makes clear that he detests Calvin Coolidge, a judgment hard to quarrel with, and he especially dislikes Andrew Peters, the Boston Police Commissioner during the strike. He blames Peters for the strike, because he had limited abilities and was rigid and unsympathetic in his dealings with the police. There are violent mobs and also a multitude of students and blundering citizens who wanted to play policeman in the strike. Russell is sympathetic to the plight of the police, although one gets the impression that because of the violence he does not condone their actions. Russell is also aware of the general forces contributing to the strike: the rising cost of living, labor unrest, and the grievances of the police. Because he concentrates on drama and personalities, one does not find a penetrating analysis of why the violence erupted in Boston.

Russell is at home in the history of the era and in Massachusetts, and he tells his story well; *A City in Terror* makes stimulating reading. One problem, however, is the light touch which begins the book, a personal, warm discussion of his father during the strike. This light touch did not seem consistent with the title of the book and much of the discussion of the violence accompanying the strike. One of Russell's conclusions may be dated. He says one of the legacies of the strike was that no American city for 50 years was threatened by a walkout of its police. With the increasing unionization of municipal employees that legacy may be ending. We now have the possibility of more police strikes, and indeed, have had some recently.

DAVID M. REIMERS  
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ROBERT W. LARSON. *New Mexico Populism: A Study of Radical Protest in a Western Territory*. With a foreword by HOWARD R. LAMAR. Boulder: Colorado Associated University Press. 1974. Pp. xiv, 240. \$10.00.

Long a favorite subject of historians, Populism has recently been the focus of a number of important studies. Historians like Nugent, Parsons, Brodhead, Clanton, Hackney, and Kouser have sharpened our understanding of the Populists, adding new dimensions and broadening the movement well beyond its original Plains-States boundaries. Wright, Bicha, and others have looked productively at Rocky Mountain Populism, especially the Colorado variety, and now Robert W. Larson has given us a richly detailed account of Populism in New Mexico.

Drawing on careful research in local newspapers and other neglected sources, Larson traces the development of the Populist movement through

the decade of the 1890s. As his analysis makes plain, the political phenomenon of Populism accomplished little in New Mexico. Populists won few elections, attracted relatively few voters, and passed virtually no legislation. As elsewhere, in its later years New Mexico Populism succumbed increasingly to the convention and electoral maneuvering. But it did embrace a wide variety of New Mexicans, far wider than previously suspected, and it had some influence on territorial development.

New Mexico Populism was a thing of extraordinary complexity; it varied from county to county, in some cases from town to town. In Lincoln and Colfax counties it reflected local protest against encroaching land and cattle monopolies. In Albuquerque merchants found it a useful means to seek better railroad rates and party reforms, while in San Miguel County antimonopoly sentiment took a special and intriguing twist, with the formation of *Las Gorras Blancas*, the White Caps, a group of Hispanic settlers who rode at night to defend their communal land holdings. Variety and localism somehow dwindled in mid-decade, to be replaced by depression, silver, and more "national" concerns. By 1898, less than ten years after its birth, Populism in New Mexico was dead.

Larson's study, with its wealth of local detail, is a welcome addition to Populist historiography. The attempts of the 1950s to provide a broad analysis of Populism left a good deal to be desired. Thanks to Larson and others, it may be time for a historian of vision to try again.

R. HAL WILLIAMS  
Southern Methodist University

ROBERT MILTON BURTS. *Richard Irvine Manning and the Progressive Movement in South Carolina*. Columbia: University of South Carolina Press. 1974. Pp. viii, 259. \$9.95.

Scion of landed gentry, Richard Irvine Manning counted among his ancestors at least four governors of South Carolina as well as prominent military figures from the Revolution through the Civil War. A successful lawyer and businessman, he entered politics, at first supporting the Bourbons against Benjamin R. Tillman and later opposing the forces of Coleman L. Blease. In 1914, after service both in the state legislature and on various Democratic party committees, Manning, with the tacit approval of Tillman, was elected governor over the Blease candidate.

The major emphasis of this volume is on the reforms initiated either by Manning as governor or by the legislature, usually with his approval. His four-year record, 1915-19, included increased appropriations for the public schools and a new com-

pulsory attendance law; a revision of the tax structure, which Manning felt was his most important accomplishment; the development of agricultural markets and an extension of agricultural education; labor legislation, which included a child labor law and a board of conciliation created to solve labor disputes; political reforms, which tightened election procedures and introduced the secret ballot in Democratic primaries for all but the smallest rural areas. Manning, the aristocrat, was able to establish a progressive record in line with other contemporary administrations in the South. He saw to it that South Carolina participated fully in World War I. Besides urging patriotism and sacrifice and fighting the antiwar activities of the Bleasites, he was able to secure army camps at Columbia (now Fort Jackson), Greenville, and Spartanburg. The naval and marine installations around Charleston were enlarged. Although these military establishments may have created problems of vice, lawlessness, and race relations in the camp areas, they "were a great boon" to the state's economy (p. 159), which perhaps was why South Carolinians eagerly sought them.

Other than noting an occasional editorial lapse, the only criticism of this volume might be that it needs a bit less of Manning and a much more extensive account of what constituted progressivism in South Carolina and of how that state's reform movement was identical to, or different from, that of her sister states. Nevertheless, the work adds a needed and interesting chapter to the historiography of the Palmetto State.

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JAMES WHORTON. *Before Silent Spring: Pesticides and Public Health in Pre-DDT America*. Princeton: Princeton University Press. 1975. Pp. xv, 288. \$12.50.

Insecticidal contamination of the environment is not a new problem. Whorton's volume chronicles that issue from the development of chemical pesticides in the 1860s to the DDT era. Insecticides such as Paris green were introduced early in this period to assist farmers with a recently intensified insect threat. These chemicals were first used at a point of great need and long before any awareness of the environmental dangers posed by them.

The first significant federal legislation applicable to the health problem raised by insecticide use was the 1906 Pure Food and Drug Act. This legislation defined illegal adulteration as the presence of poison or deleterious ingredients which rendered an article injurious to health. The federal regulatory problem, henceforth, was to detect such

substances, define danger levels, and make these decisions stick legally. New food and drug legislation was enacted in 1938 after five years of legislative efforts, a struggle which both opened and closed with the issue of pesticide residue on fruits. This act strengthened federal controls but in no sense solved the pesticide problem. Indeed new insecticides, principally DDT in the post-World War II years, would prove more dangerous than previously popular lead arsenate, because they were more destructive to the environment in which they were used.

While actions by states have been slighted, this volume is an excellent one. It offers a balanced, sophisticated treatment of a problem which has often been subjected to oversimplification and unproductive, passionate indictments. Moreover, if most consumers are aware of charges that their foods are tainted with pesticide residue, they may be surprised to read that their grandparents faced the same hazards. The historical perspective should provide a valuable corrective for a popular inclination to see this issue solely in contemporary terms.

CHARLES O. JACKSON  
*University of Tennessee*

MARK H. ELOVITZ. *A Century of Jewish Life in Dixie: The Birmingham Experience*. University: University of Alabama Press. 1974. Pp. viii, 258. \$10.00.

It is true, as Jacob R. Marcus suggests in the foreword, that American Jewish historiography is still embryonic and Southern Jewish history even more so. Marcus is undoubtedly justified in claiming that Mark H. Elovitz's volume on Birmingham, Alabama, ranks as "the first substantial history of the Jews in any inland town or city of the industrial South." We have histories, all either fragmentary or otherwise inadequate, of Southern Jewish communities like Charleston, South Carolina, Nashville, Tennessee, and New Orleans, but Elovitz is the first to attempt an account of Jewish communal development in a new, that is, post-bellum, city like Birmingham.

In many respects, his work is admirable. He brings the history of Birmingham Jewry up to the present, offers his readers a description of the changing socioeconomic and religio-cultural scene, does not avoid divisive factors or controversial issues such as Jewish attitudes toward the desegregation conflict of the 1960s, documents his study well, and supplies some useful appendices. He is aware, too, that Birmingham Jews have not lived in a vacuum and is at some pains to connect the city's Jewish experience with the experience of the larger non-Jewish community. He also documents the ethnic bifurcation of Birming-

ham Jewry: from the earlier Central European settlers came the important merchants and investment bankers of the Phoenix Club and Highland Avenue; the East Europeans who arrived a few decades later created on Birmingham's north-side an area "so Jewish that 'you could tell and smell Friday night because everyone was dressed up [for the Sabbath] and what was cooking pervaded that several block area.' " A half-century passed before the two groups fused. Birmingham lags behind in this respect, but the Jews do not lag behind where mercantile activity is concerned: as recently as the mid-1960s six of Birmingham's top seven general merchandising firms were Jewish-owned. Still, despite their prominence in commercial and civic life, the Jews have not been at one with their non-Jewish neighbors. According to one of Elovitz's confidential sources, the Jewish community leadership views the desegregation fight as a "Christian problem" between whites and Negroes and "not simply a racial problem."

Elovitz describes, or perhaps one ought to say chronicles, the Birmingham Jewish experience. He appears, however, to work without any conceptual framework. The reader knows something of what has happened in Birmingham; he is told virtually nothing as to why it happened. His book is heavy with data (for which future historians will surely be grateful). But what these data mean, what they tell us historiographically, is apparently no concern of the author's. That diminishes the value of his work.

STANLEY F. CHYET  
*Hebrew Union College*

RALPH ELDIN MINGER. *William Howard Taft and the United States Foreign Policy: The Apprenticeship Years, 1900-1908*. Urbana: University of Illinois Press. 1975. Pp. xii, 241. \$9.50.

William Howard Taft had a significant role in United States foreign policy even prior to his election to the presidency. He headed the Taft Commission to the Philippines in 1900 and then served as governor there. After becoming Secretary of War in the cabinet of Theodore Roosevelt, he had Panamanian affairs under his jurisdiction. Roosevelt also dispatched him to Cuba to deal with political unrest there in 1906. Meanwhile, he had undertaken the first of two trips to the Far East. In 1905 he visited Japan, the Philippines, and China. Two years later he visited the same countries and proceeded around the world via Russia.

In recounting these events Minger draws primarily upon the papers of Taft, McKinley, and Roosevelt. The story from this limited range of sources is already known to many readers. Most of the chapters of this book were published as

articles in the early 1960s. They are now republished with little revision, and therefore do not come to grips with interpretations of scholars who have published in the last decade and a half. There are no references to the studies made by Richard D. Challener, Michael H. Hunt, Akira Iriye, Allan R. Millett, Charles E. Neu, and Peter W. Stanley. The book also has serious editorial deficiencies, such as the material on pages 7–9 being repeated verbatim on pages 183–185. Minger's study leaves much to be done on Taft's role in foreign policy in the period 1900–1908.

RAYMOND A. ESTHUS  
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JOHN DEWITT MCKEE. *William Allen White: Maverick on Main Street*. (Contributions in American Studies, number 17.) Westport, Conn.: Greenwood Press. 1975. Pp. x, 264. \$12.95.

John McKee has written a brief, engaging biography of William Allen White (1868–1944), the small-town Kansas editor and progressive reformer who became one of the esteemed figures in American journalism during the early twentieth century. The book has several distinct virtues. It is readable; it reflects careful research in White's published and unpublished materials; and it seeks to interpret as well as to describe White's fascinating career.

A recurring theme is that White sought continually (and with considerable success) to make his "private sentiment public opinion." McKee stresses that White's maverick qualities always rested on a solid core of beliefs, including the assumption that conduct was proper insofar as it reaffirmed the middle-class respectability of village America. Such a view allowed him to rebuke Populist malcontents who seemingly challenged that world and, subsequently, to cheer for Main Street progressivism that sprang from the right kind of people. Predictably, his great hero was Theodore Roosevelt, who exuded respectability while reaffirming the righteous individual's impact on society. To the end, White kept his faith in a moralistic universe, full of promise, imbued with Christian ethics, and responsive to the demands of justice and responsibility. As McKee argues, White was "one of the best lay preachers ever to use the editorial page as a pulpit" (p. 27).

Although McKee's narrative is interesting, it should not surprise historians long familiar with White's masterful account of his own life. *The Autobiography of William Allen White*—which contributed substantially to White's historical significance—confronts his biographers with the unenviable task of supplying more insights into the man than he himself so gracefully provided. There

are ways around the problem, however, as Robert Bannister demonstrated in his excellent study of Ray Stannard Baker, another progressive journalist who, like White, wrote a major autobiography. McKee could have drawn considerably more from secondary materials and related manuscript collections; he could also have been more attentive to historiographical issues (for example concerning the nature of progressivism and its relationship to the twenties and thirties). By overlooking many relevant secondary sources, and by generally skirting historiographical matters, McKee missed a chance to write a more substantial book.

LEROY ASHBY  
Washington State University

FLORETTE HENRI. *Black Migration: Movement North, 1900–1920*. Garden City, N. Y.: Anchor Press/Doubleday. 1975. Pp. xi, 419. \$9.95.

THOMAS SOWELL. *Race and Economics*. New York: David McKay. 1975. Pp. ix, 276. Cloth \$9.95, paper \$4.95.

*Black Migration: Movement North, 1900–1920* delivers both more and less than its title suggests. It is not the careful, detailed examination of the migration process and its consequences—especially of the Great Migration—that scholars so badly need. It is a survey of race relations and conditions of black life, urban and rural, North and South, in the first two decades of this century. "In 1900," Florette Henri observes, "black people were practically invisible on the American scene; by 1920 they were visible, audible presences everywhere in the national life. How and why did it happen?" (p. vii). These questions are answered competently and intelligently on the basis of a fairly thorough survey of existing scholarship and published sources, although there are curious omissions of important, recent monographs. The only original research in unpublished sources relates to the situation of blacks in the military, a subject on which Henri has previously published. The book will be useful to the student seeking a general overview, but it will offer no special surprises or new insights to the scholar.

Thomas Sowell takes one of the themes in Henri's narrative—economic opportunity and achievement—and examines how it has been shaped historically by race and ethnicity. Studying the economic development not only of blacks but also of Jews, Irish, Italians, Japanese-Americans, West Indians, Puerto Ricans, and Mexican-Americans, Sowell investigates the impact on the socioeconomic position of the various groups of such factors as "color, language, nationality" (p. 60), religion, "time of arrival in America," and "the

time of movement . . . into a modern, industrial, and commercial economy . . ." (pp. 34-35). He focuses especially on the relationship between discrimination and economic progress, and he argues provocatively, for example, that economic advancement has had little to do with tolerance; that "the lessening of group animosity has not been a precondition for economic advancement, and rapid economic advancement has often been accompanied by increased group animosities" (p. 162); that "the current disabilities of black Americans are due not only to current discrimination but also to past deprivation and disorganization that continue to take their toll" (p. 102); that "low-income origins, overcrowded and substandard housing, prejudice and discrimination, inadequate educational opportunities, and a general failure of public services . . . to do their jobs properly in minority neighborhoods" all "impeded the progress of all American minorities; but it is by no means clear that the more successful minorities had any less of such handicaps than the less successful minorities" (p. 143). Sowell concludes that "how long a group's culture has been an urban, industrial, and commercial one is a key factor in its success in urban, industrial, and commercial America" (p. 149); that "current income differences between ethnic groups" can be explained in part by the difference in "the average age of the respective groups" (p. 150); that "a general standardization of jobs, wages, and promotions patterns has also made it harder for contemporary disadvantaged minorities to advance than for their nineteenth-century counterparts" (p. 154). Sowell holds, therefore, that those who castigate blacks and Puerto Ricans for failing to pull themselves up by their own bootstraps in the manner of Irish or Jewish immigrants of the last century are ignoring a whole complex of cultural and economic factors that need to be understood in historical perspective. Florette Henri offers such a perspective.

NANCY J. WEISS  
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BURTON I. KAUFMAN. *Efficiency and Expansion: Foreign Trade Organization in the Wilson Administration, 1913-1921*. (Contributions in American History, number 34.) Westport, Conn.: Greenwood Press. 1974. Pp. xviii, 300. \$12.50.

ARTHUR S. LINK *et al.* *Wilson's Diplomacy: An International Symposium*. (The American Forum Series.) Cambridge, Mass.: Schenkman Publishing Company; distrib. by General Learning Press, Morristown, N.J. 1973. Pp. xiv, 120. Cloth \$7.65, paper \$4.50.

These two volumes, one a research monograph and the other a collection of essays by inter-

nationally known historians, add significantly to the knowledge about Woodrow Wilson's presidency and point to areas in need of additional research. Kaufman argues convincingly that "the Wilson administration fundamentally altered American foreign commercial policy by coordinating and integrating foreign trade activity and, in the process, by defining new business-government relationships that continued to exist even after Wilson left office in 1921" (p. xiii). Such a policy, Kaufman notes, points out some of the contradictions in Wilsonian thought, for while Wilson advocated the principles of laissez faire and free-marketplace competition, his administration assumed a much greater role in "directing the nation's foreign trade forces, even going so far as to exempt business combinations engaged in foreign trade from the provisions of the antitrust laws" (p. xiii).

Kaufman's study builds on the works of Gabriel Kolko, Robert Wiebe, Paul A. Koistinen, James Weinstein, Robert Cuff, Carl Parrini, and Joan Hoff Wilson. All these authors have examined the changing nature of the American political economy in the Wilson years and noted the cooperation between government and business, especially because of World War I. But some of them, according to Kaufman, place too much stress on the differences of strategy for economic development between the government and certain sectors of private business, and as a result they seriously underestimate "the coherence of purpose, the breadth of accomplishment, and, indeed, the historical uniqueness that characterized business-government efforts at foreign trade organization during the Wilson years" (p. xvi).

Kaufman concentrates on the organization of foreign trade, pointing to the frequent cooperation between government and business. Of special interest are the discussions of Wilson's personal views on foreign trade; of the influence that the dramatic growth of German trade, aided by close cooperation between the German government and merchants, had in America; and of the intensive rivalry between the United States and Great Britain for trade supremacy in Latin America before, during, and after the war.

Kaufman notes that there were problems in the relationship between business and government that had not been resolved by 1921. The Edge Act proved inadequate to meet the medium- and long-term investment requirements of American overseas commerce. The depression of 1920-1921 brought a contraction of American banking operations overseas. And neither the attempt to develop an inland-waterway network in the United States nor the effort to construct an effective American merchant marine proved successful.



Nonetheless, business and government leaders found vast areas of agreement by the end of the Wilson presidency. "Never before," Kaufman writes, "had business and government worked so closely in developing overseas markets; never before had an administration assumed such an active role in leading and directing the nation's foreign trade forces" (p. 262).

*Wilson's Diplomacy: An International Symposium* is a collection of essays and rejoinders by Arthur S. Link, Jean-Baptiste Duroselle, Ernst Fraenkel, and H. G. Nicholas. The editors hope that, by bringing scholars from the United States, France, Germany, and Great Britain together, a more complete picture of Wilson's contribution in international relations will emerge. The result is not altogether successful, as the four essays have no common theme. Yet, some of the essays are interesting.

Link's essay is a revision of one of the Albert Shaw Lectures he delivered at The Johns Hopkins University, published in *Wilson the Diplomatist, a Look at His Major Foreign Policies* in 1957. The most fascinating aspect of Link's essay is how his view of Wilson has changed in recent years, especially regarding Wilson's knowledge of the influence of economics upon American diplomacy. Clearly, recent historiography has helped Link reach the conclusion that Wilson was much more knowledgeable about economics than previously realized, and Kaufman's study will probably reinforce Link's view on this subject.

The Duroselle and Fraenkel essays discuss Wilson's reputation in France and Germany. The Duroselle essay measures Wilson's contemporary popularity in France, especially in newspapers. Unfortunately, Duroselle's essay has too little analysis. The Fraenkel essay is much better, showing Wilson's emergence in Weimar Germany as a scapegoat for Germany's troubles. While Wilson was soon forgotten in France, he remained a dominant figure in German history throughout the 1920s and 1930s, and the causes for this difference merit further research. The least successful essay is that by Nicholas, which stresses the weaknesses of Wilson's diplomacy. The book concludes with rejoinders from Link, Duroselle, and Nicholas.

Specialists on the Wilson presidency and on American foreign relations will find much of value in these two very different volumes.

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YVES-HENRI NOUAILHAT. *Les Américains à Nantes et Saint-Nazaire, 1917-1919*. Preface by PAUL BOIS. (Annales littéraires de l'Université de Nantes, number 4.) Paris: Les Belles Lettres. 1972. Pp. 250.

Nantes and Saint-Nazaire were the main war matériel entry ports for the American Expeditionary Force. Yves-Henri Nouailhat has written a model history of the resulting flood of 3,300,000 tons of equipment and supplies and, not counting transients, some 60,000 men. Engineering problems were handled very well. But mutually reassuring official visits—carried to the point of obsequiousness by both sides—did little to curb French working-class irritation over prices, property damage, liquor, women, American trigger-happiness, and M. P. brutality, especially toward their own black stevedores. Thousands of American motor vehicles pot-holed the roads, caused monstrous traffic jams, killed four people and five horses, and hit thirty-four horsedrawn and forty-eight motor vehicles, thirty-four bicycles, nine wheelbarrows, and seventy-six streetcars in Nantes alone. But bad memories seem to have been as ephemeral as the great expansion of American trade through the ports. When the Germans destroyed Mme. Whitney's monument to American aid, the "American presence" in this part of Western France had become "a golden age."

THEODORE ROPP  
Duke University

NORRIS HUNDLEY, JR. *Water and the West: The Colorado River Compact and the Politics of Water in the American West*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xxi, 395. \$20.00.

In the history of the American West, no resource has been more precious than water. Because of its scarcity, Westerners have historically used it with greater care than their better endowed Eastern counterparts. Norris Hundley's impressive monograph treats the West's major effort to agree on equitable usage of this vital but limited commodity: the Colorado River Compact of 1922. The book posits that growing federalism finally overshadowed the attempt of seven Western states in the Colorado River Basin to solve their water problems. Using the 1922 compact as the focus of his study, the author devotes appropriate attention to antecedent problems, to negotiation of the compact under the leadership of Secretary of Commerce Herbert Hoover, and to its ratification. In his analysis, Hundley deals with the compact's implications for Mexico since the river reaches the sea through her territory. While American negotiators observed Mexico's legitimate interests, they strove not to augment any Mexican claims to waters of the Colorado. Although in 1922 they paid scant attention to the rights of reservation Indians to water, later adjudication established these rights. Because of decreased flow of the streams,

however, Indians have never irrigated as many acres as the Supreme Court said they could.

Prodigious research went into this monograph, as is reflected in the thorough, even heavy, documentation. While the author's admirable organization and clear prose provide ample thrust and movement, his extensive research has led him to include more information than necessary to establish his points, particularly in the chapter dealing with ratification of the compact in the seven involved states: Utah, New Mexico, Wyoming, Colorado, Nevada, California, and Arizona.

A major strength of the volume is the lucid analysis of complex legal and political aspects of water law and usage. Nowhere have I seen a clearer exposition of the conflicting water laws of the West—common law riparian rights and the doctrine of prior appropriation. Hundley has made a significant contribution to a difficult subject. In doing so, he demonstrates the kind of sophisticated analysis needed to provide understanding of the American West.

WALTER RUNDELL, JR.  
*University of Maryland*

JOHN DOUGLAS FORBES. *Stettinius, Sr.: Portrait of a Morgan Business Partner*. (Publications of the Graduate School of Business Administration of the University of Virginia.) Charlottesville: University Press of Virginia. 1974. Pp. xii, 244. \$12.00.

This biography of Edward Stettinius, Sr., business executive and later partner in the J. P. Morgan investment banking firm during the First World War, is a disappointing book. The author may be only partially responsible for the outcome. One strongly suspects that John Douglas Forbes, a member of the Virginia faculty, was recruited to write a volume honoring the father of one of the university's most famous alumni, Edward Stettinius, Jr., Secretary of State under FDR, following the very recent donation of family papers to the university's manuscript collection. As Forbes candidly admits, there were grave misgivings from the outset whether sufficient material was extant on the senior Stettinius to prepare a genuinely scholarly book. There was not.

The available material has been stretched to the limit. There is much padding; we are frequently given lengthy samples of unedited correspondence, which often contains as much trivia as substance. Also included in the text are such items as news clippings from the society pages of the *New York Times* recounting the activities of the smart Long Island set to which the family belonged in the twenties. Much of this sketchy material is little more than memorabilia. Furthermore, the data are heavily biased. An indefatigable worker, Stet-

tinus was truly generous with his time and money, and friends and associates invariably depicted him in glowing terms. Undoubtedly much of the praise was merited, but the author seems largely unaware of the danger of constantly repeating his subject's virtues; many readers will probably receive the impression that this book was intended as another sympathetic rehabilitation of a prominent member of the Wall Street establishment. Stettinius had the misfortune to be posthumously linked to that imaginary group of businessmen the Nye committee maliciously labeled "merchants of death" in the 1930s, and while Forbes labors to absolve the Morgan partner from this cruel allegation, he overdoes it.

Despite the paucity and inherent bias of the primary sources, the author might have improved the book substantially if he had provided a better framework for discussing Stettinius' activities as the American purchasing agent for the British and French during the first two years of World War I and later as a member of the American mobilization (and demobilization) team. For example, there is no evidence that he consulted the important work of Robert Cuff or Paul Koistinen on the complexities of business-government relations during the war period.

EDWIN J. PERKINS  
*University of Southern California*

AL-TONY GILMORE. *Bad Nigger! The National Impact of Jack Johnson*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 162. \$9.95.

RICHARD EDWARD LAPCHICK. *The Politics of Race and International Sport: The Case of South Africa*. (Center on International Race Relations, University of Denver. Studies in Human Rights, number 1.) Westport, Conn.: Greenwood Press. 1975. Pp. xxx, 268. \$13.95.

Historians have never made much use of sport as a vehicle for social analysis. Yet in countries like the United States and South Africa where sport is an important public activity intimately linked with matters of race and class, the potential for doing so is obvious. Traditionally, the ethos of sport has operated to obscure the political and sociological dimensions of athletics, but this is no longer the case. The use of recent Olympic games as a political forum by groups as diverse as black Americans, Palestinian terrorists, and cold warriors from both sides of the iron curtain is but one illustration of the close and growing union between sports and politics.

The union is hardly new, as the two books under review demonstrate. If neither book is felicitously written, both are informative and useful for histo-

rians and sociologists. Based primarily on newspaper sources, Al-Tony Gilmore's *Bad Nigger!* focuses on public reactions in white and black America to the activities of Jack Johnson, the first black heavyweight boxing champion. The years of Johnson's championship, 1908-15, were years of great difficulty for all blacks, including athletes. Jim Crow reigned in the boxing ring as elsewhere; white titleholders refused to fight black challengers. In 1908, however, heavyweight champion Tommy Burns accepted a challenge from Jack Johnson in hopes that an interracial fight would revive interest in boxing and incidentally earn him some money.

Johnson defeated Burns handily and proved at once to be a singular champion in and out of the ring. He was not what whites called "a credit to his race." He flouted the customs of Jim Crow, married three white women and had liaisons with many others, and defeated a succession of "white hopes." His greatest victory, over Jim Jeffries in 1910, occasioned racial clashes across the nation; his preference for white women provoked white officialdom to accuse and convict him of violating the Mann Act; and his very example caused Jess Willard, who defeated him in disputed circumstances in Havana in 1915, to restore Jim Crow to the ring. In describing public reaction to these events, Gilmore illustrates the political uses of sport, including the pride blacks took in Johnson's accomplishments and the concern Johnson aroused in conservatives like Booker T. Washington. *Bad Nigger!* is too short on analysis to constitute the final word on any of these subjects and too neglectful of Johnson's personal views on racial topics, but it does demonstrate the public impact of Johnson's championship.

*The Politics of Race and International Sport*, a more substantial work than *Bad Nigger!*, is a detailed, chronological account of the effort, between 1959 and 1970, to get South Africa expelled from the Olympic games and other international sports competition because of its practice of racial apartheid in selecting competitors. The story of this effort can only be described as incredible. The International Olympic Committee and other sports bodies were simply unwilling to enforce the Olympic rule that competitors must be selected solely on the basis of athletic merit. Lapchick shows convincingly that this was due to the dominance of international sports bodies by whites who were willing to tolerate discrimination against blacks in South Africa for racial and political reasons. He shows just as convincingly that expulsion succeeded for the same kinds of reasons. By the 1960s nonwhite athletes could no longer be ignored in international competition, nor could the non-white world be ignored politically. Perhaps the

most revealing aspect of this story is that white South Africans tried to avoid expulsion from international competition by making the first minor, cosmetic concessions they had ever made to critics of apartheid. Sport was that important to them.

I. A. NEWBY  
University of Hawaii

JIM POTTER. *The American Economy between the World Wars*. A Halsted Press Book. New York: John Wiley and Sons. 1975. Pp. 183. \$10.50.

Jim Potter wrote this "introductory guide" primarily for British readers. In his words, "it does not set out to be comprehensive, but is intended to provide students with a fairly simple factual outline and with suggestions of differing interpretations." The heart of the book consists of a chapter on the common features of the interwar years, successive chapters on the subperiods 1922-29, 1929-33, 1933-39, and a final chapter assessing the accomplishments of the New Deal. A great deal of quantitative evidence appears in the twenty-eight text tables. Appendices include a chronology of events, a glossary of abbreviations, and some suggestions for further reading.

The writing is clear and concise, the major conclusions fairly compelling. Potter recognizes that the central issue is not the occurrence of a depression in the 1930s but rather its extraordinary severity and duration. He concludes that nothing in the 1920s made a Great Depression inevitable; that the Great Depression reflected a collapse of confidence in the future profitability of private investment; that the New Deal, an expedient and inconsistent set of policies for the attainment of a wide variety of objectives, failed in its recovery efforts precisely because it never succeeded in reviving business confidence; and finally, that the great significance of the New Deal arose from its enduring institutional reforms, not from its relief or recovery programs.

Potter's analysis takes an eclectic form, placing heavy emphasis on the social, political, and institutional context of the economic events. Such an approach has considerable virtues, yet this very eclecticism involves the author in some dubious economic theorizing (e.g., in his indictment of America's "anachronistic" institutions). In a section on "suggested explanations of the Great Depression" I was astounded to find only one sentence concerning the role of monetary policy in the early 1930s but three pages on the long-defunct stagnation hypothesis. Looking back over my copy, I see the margins littered with question marks, exclamation points, and other symbols of discontent, most of them summarized in my general feeling that the text could benefit from a more

careful attempt to integrate its analysis with modern macroeconomic theory.

Despite these limitations, the book generally succeeds well enough on its own terms, as "no more than a summary account, often of exceedingly complicated matters."

ROBERT HIGGS  
*University of Washington*

BURL NOGGLE. *Into the Twenties: The United States from Armistice to Normalcy*. Urbana: University of Illinois Press. 1974. Pp. ix, 233. \$8.50.

Through a series of gracefully written essays on familiar topics Noggle attempts "to synthesize the numerous and discrete ideas and events" in post-World War I America "that spawned the phenomenon labeled 'the twenties'" (p. vii). His work deftly combines a wide range of secondary sources with selected manuscript research. Three brief chapters on reconstruction offer the most interesting and unfamiliar material, all suggesting that a disinterested president and a divided, apathetic Congress squandered the opportunity for postwar reform and revitalization. The chapter on the Red Scare attempts unsuccessfully to attribute more reasonableness to antiradicalism than has generally been conceded. The chapter on diplomacy departs from the narrative mode to present a useful historiographic essay. The penultimate chapter on American society is a polite bow to the existence of social history. The final chapter, "Remnants of Progressivism," does not explore the complex transformation of progressivism but surveys postwar politics from the perspective of several political figures.

The book, unfortunately, sidesteps the complex problems of historical synthesis and explanation. Lacking a sustained thesis, Noggle has no systematic basis for selecting and emphasizing different issues. Why, for example, devote thirty-eight pages to the Red Scare and thirty pages to foreign policy when issues related to urbanization, race, women, prohibition, leisure activities, religion, education, and scholarship are all crammed into twenty-seven pages? Except for the section on reconstruction, each chapter is a watertight compartment sealed off from the rest of the book. The work neither cumulates insights nor offers variations on an underlying theme. It even fails to include a summary or conclusion.

If Noggle had actually explained aspects of the twenties by reference to ideas and events in the postwar period, he could have established grounds for selection and emphasis and attained the unity and direction necessary for historical synthesis. Yet he short-circuits successful explanation by fail-

ing to specify those features of the 1920s that he wishes to understand. He mentions only such vague tendencies as "weariness," "apathy," "reaction," "chauvinism," and "disillusion," virtually caricaturing the decade. Noggle's argument about the failure of political leaders to exploit opportunities for reform could have evolved into a persuasive causal explanation, but it is neither thoroughly developed nor extended throughout the work. Noggle, moreover, never examines alternative hypotheses or critically assesses their logical and empirical validity.

The author appears to have scaled the historical mountain of post-World War I America simply because it had not been done before. The climb, however, has provided neither a coherent perspective on the period nor any new eye-catching insights or information. But he does present material that is not readily available elsewhere about a generally neglected period. I hope that his work will stimulate more systematic research.

ALLAN J. LIGHTMAN  
*American University*

ARTHUR E. MORGAN. *The Making of the TVA*. Buffalo: Prometheus Books. 1974. Pp. xiv, 205. \$10.95.

Arthur Morgan was the first chairman of the Tennessee Valley Authority, and this volume, written in Morgan's ninety-sixth year, is an account of both his years on the Authority and his life as an engineer and social planner.

A generous portion of the book is devoted to the conflict between Morgan and his antagonists on the Authority, David E. Lilienthal and Harcourt Morgan. For Arthur Morgan, it was primarily a situation where his "ethical principles" and devotion to public service were arrayed against the "politics-as-usual" and patronage of his colleagues. Morgan, who wanted to use the TVA to enhance the overall quality of life for the people of the region, ultimately realized that his hopes were doomed when President Roosevelt succumbed to the more limited vision and public-power pre-occupations of Morgan's foes. In 1938, accused by his colleagues of being an impractical visionary whose dreams were hindering the development of public power, Morgan was dismissed by the president after failing to substantiate his own charges that his fellow directors were guilty of deceit and dishonesty.

Morgan also describes his long and distinguished life in engineering. Among other accomplishments, he includes his introduction of "dynamic design," which consisted of never finally completing a design "but of keeping it open to take full account of any special advantage or disadvantage that appeared in the course of construction"

(p. 95). Although criticized by others who misunderstood the approach, Morgan claims that it not only saved money but also put unemployed men to work sooner.

Despite his personal frustrations during those years, Morgan considers them among the most significant and rewarding of his long life. He insists that, at last, his broader concept of what the TVA ought to be has been realized through many of the Authority's projects in recent years.

THOMAS H. COODE  
California State College,  
Pennsylvania

R. J. C. BUTOW. *The John Doe Associates: Backdoor Diplomacy for Peace, 1941*. Stanford: Stanford University Press. 1974. Pp. x, 480. \$16.95.

For a full year preceding Pearl Harbor, Father John Drought attempted to influence the negotiating positions of the Japanese and American governments. Joining Drought as the "John Doe Associates" were his superior, Bishop Walsh, Postmaster General Frank Walker, a Japanese banker Tadao Wikawa, and a highly placed officer of the War Ministry, Colonel Hideo Iwakuro. On the periphery hovered Prince Konoye and Yosuke Matsuoka, Roosevelt and Hull. Under Drought's guidance the associates produced: 1) a "working analysis" to rally support among Japanese expansionists for a diplomatic overture to Washington; 2) a vaguely worded draft agreement; and 3) a proposal for a head of government summit (the Roosevelt-Konoye meeting which never took place).

Did these proposals or their authors have much backing from either government? Since the Tokyo War Crimes trials revealed the existence of the associates, this unsolvable mystery has bedeviled historians. The possible answers are many and complex. Some or all of the associates may have been direct agents, others only private parties; even if some were agents, the degree of government control may have been slight.

Butow provides a simple answer: The well-meaning associates were all amateurs, acting on their own, who succeeded in convincing both governments that they represented the "other side." Since the associates tended to minimize differences between Washington and Tokyo, the deception was most unfortunate. When the true position of each government was revealed, it seemed as if the gap had widened. Further negotiations seemed pointless. Both sides prepared for war.

Butow's theme, that amateur diplomats rarely advance peace, is sound, but his indictment is much too harsh and simple. Clearly, officials in both governments decided at some point to use

these private channels to seek a settlement precisely because they were disavowable. It is equally clear that the secrecy used to render them disavowable has hopelessly obscured the circumstances of governmental involvement. (If the evidence points in any direction, it is toward Iwakuro and Walker as the real agents, two figures who never clearly emerge in Butow's narrative.) But it is unlikely that either government much trusted the associates as a group or was much misled by their representations.

Butow's accusation that Drought spoiled any chance of a settlement (p. 320) seems particularly unfair coming from an author who does not really believe peace was possible and who applauds Hull's consistent rejection of any compromise peace as unacceptable appeasement (pp. 295, 301). But the United States, having failed to challenge one Japanese fait accompli after another, could only have avoided war in 1941 by some degree of appeasement, as Drought implied—and as the abortive American *modus vivendi* acknowledged. When Hull rejected appeasement, war was inevitable; this was probably the right decision but not better "diplomacy." Clinging desperately to peace, Drought was a minor figure in this tragedy.

While Butow's handling of Japanese sources is unmatched, and his insights into the intricacies of negotiations are often brilliant, he follows Hull too closely, even to the point of assuming Hull's prosecuting manner. Historians do well to avoid such a role.

BRIAN LORING VILLA  
University of Ottawa

JOSEPH W. ESHERICK, editor. *Lost Chance in China: The World War II Despatches of John S. Service*. New York: Random House. 1974. Pp. xxviii, 409. \$12.95.

This book contains some seventy-one dispatches and memoranda written by John Service, a Foreign Service career officer stationed in China from the fall of 1935 to his recall during the spring of 1945 amidst governmental charges of pro-Communist sympathies, of which he was eventually cleared in 1957 by the Supreme Court.

The documents, twenty-six of which have never been published elsewhere, constitute a valuable addition to the voluminous literature on U.S.-Chinese relations during the last years (1941-1945) of World War II, a period that witnessed the increasing deterioration of the Kuomintang and the dynamic growth of the Chinese Communist movement. The chief virtue of these reports appears to lie in John Service's vivid and perceptive observations in his capacity as a member of the U. S. Army Observer Section to the Chinese Communist



headquarters at Yenai in 1944-1945. Service's assignment was to report on the political and economic conditions prevailing in the Communist-controlled border region and on policy statements of the Chinese Communist leaders such as Mao Tse-tung and Chou En-lai. In view of the current diplomatic impasse between the United States and the People's Republic of China, it is of vital interest for those scholars and experts studying U. S.-China relations, as well as for policy-makers in Washington, to reexamine these incisive dispatches. Indeed the "Lost Chance in China" is still to be regained!

Unfortunately, the arrangement of the documents leaves something to be desired. Grouped under three broad topic headings, *Kuomintang China*, *The Communist Areas*, and *The Debate with Hurley*, they have no strict chronological order or centralizing theme to tie the various parts and headings together. There are perhaps too many repetitious passages and *non sequiturs*. But the editor deserves credit for his succinct notes which are interspersed throughout the book.

J. CHESTER CHENG  
San Francisco State University

THOMAS C. KENNEDY. *Charles A. Beard and American Foreign Policy*. Gainesville: University Presses of Florida. 1975. Pp. xi, 199. \$8.50.

Thomas C. Kennedy has exhaustively and painstakingly examined all of Charles Beard's writings and all writings about him. The author has also made extensive use of manuscript collections, and he has garnered information from various private sources for fifteen years. He is particularly interested in appraising Beard's later views on American foreign policy, and his book is a tour of the historiography of the late forties and the fifties. While Kennedy concedes some of the details of Beard's indictment of the "interventionist" diplomacy of 1939-45, he endorses the consensus which "interventionist" scholars had established about Beard's work by the middle fifties: stemming from a doctrinaire, isolationist viewpoint, Beard's later scholarship is that of a publicist and not a detached scholar.

Although Kennedy's book is conscientious, it is not informed by interpretive penetration. The "detached scholars" Beard is implicitly contrasted to are such State Department partisans as Herbert Feis, William L. Langer, and Arthur Schlesinger, jr. If Kennedy must evaluate the truth of Beard's history, fairness to Beard demands that his work be measured against the available sources rather than the writings of historians of the fifties whose books were never out of step with their liberal internationalist politics. More importantly, Ken-

nedy fails to appreciate the central importance of Beard's belief that American involvement in war was destructive of reform at home. Certainly this belief led Beard to lose his critical sense in attacking Franklin D. Roosevelt's war policies: tormented by what he feared would be the domestic consequences of the war, Beard made FDR the chief villain of the 1939-41 period; for Beard the executive's failure to solve the problem of depression led to adventure abroad. Beard's views on New Deal diplomacy are a parody of the truth, but this is insufficient warrant for dismissing his understanding of the link between foreign and domestic policy. Kennedy treats this belief as an *idée fixe*, all but ignoring recent scholarship on the cold war. But surely time is past to examine whether causal connections exist between the diplomacy of 1939-49 and the events that terrified Beard—the loss of civil liberties, the regimentation of thought, the growth of a monstrous military establishment, and the large-scale and deliberate deception of the American people by a president of the United States.

Charles Austin Beard left all historians of the Republic in his debt. He still needs a study that is both diligent and critically intelligent.

BRUCE KUKLICK  
University of Pennsylvania

DARWIN PAYNE. *The Man of Only Yesterday: Frederick Lewis Allen*. Foreword by RUSSELL LYNES. New York: Harper and Row. 1975. Pp. xi, 340. \$12.50.

This biography deals more with Frederick Lewis Allen's career as magazine editor than with his work as popular historian. Educated at Groton and Harvard, Allen served apprenticeships with the *Atlantic Monthly* and *Century* before joining the staff of *Harper's Magazine* in 1923. As editor from 1941 to 1953, he guided that periodical through some of its finest years. Payne praises Allen for not attempting to woo a mass circulation and building up instead a loyal readership among thoughtful people. Although Allen published distinguished stories and poems, he gave much more space to provocative articles. He had a flair for developing new writers. He defended intellectual freedom by resisting the pressure of Senator McCarthy and other pseudopatriots.

Busy though he was, Allen found time, mostly evenings and weekends, to write *Only Yesterday* and other best sellers. It is easy to criticize these. Allen relied mostly on secondary materials. He dealt too much with the fascinations of collective behavior—changing styles, popular hobbies, and the like—and not enough with major problems, such as the power structure, the frustrations of various groups, and the blight of poverty. Yet his work was never

cheaply popular. Written in a sparkling style, it challenged the reader to think about the significance of recent events.

Payne's biography is solid rather than brilliant. It provides useful information about the magazine world and the impact of two world wars. But it is not as penetrating as one might wish in probing the virtues and deficiencies of Allen's genteel liberalism.

NELSON M. BLAKE  
*Syracuse University*

D. CLAYTON JAMES. *The Years of MacArthur*. Volume 2, 1941-1945. Boston: Houghton Mifflin Company. 1975. Pp. xix, 939. \$15.00.

With his second volume of the MacArthur biography, D. Clayton James continues to meet the high standards of careful research and scholarly writing set in his study of the Pacific commander's earlier career. The volume covers more familiar ground than the first book, but it is of great significance for the student of World War II.

The author has drawn heavily on official histories of the war in the Philippines and the South and Southwest Pacific. He has added to this material extensive study of the MacArthur papers and those of many of his contemporaries, has restudied official papers used by others, and has interviewed scores of the general's former subordinates and colleagues. He is restrained in his judgments and fair in his assessments of blame.

The picture of MacArthur is a more fully fleshed-out sketch of the character so admirably portrayed in the first volume—a highly controversial leader who was convinced of his special gifts for strategic thinking and high command and of his special destiny. Certain that proper strategy required greater attention to operations in the Pacific rather than in Europe, he was quick to assume that any failure to follow his lead was evidence of a plot against him in the White House and War Department. His frustrations resulted in attacks on the Navy, the Joint Chiefs of Staff, and the British.

On several occasions he reminded General Marshall that the people of the United States would not like to be told that the British were dominating American policy or that he was deprived of the opportunity to hasten the defeat of the Japanese. In such a state of mind he failed to discourage suggestions that he run for the presidency. Through the fall of 1943 and into the first few months of 1944, some newspapers spoke of him as Roosevelt's McClellan, and some members of Congress proposed that he be made super-commander of the armed forces.

James gives full credit to MacArthur for rallying the people of Australia, for backing General Ken-

ney in his air developments in the Southwest Pacific, for full cooperation with Admiral Halsey, for the use of air supply of troops and end-run amphibious operations to press his attacks. But James also raises questions about the failure to disperse planes at Clark Field long after MacArthur knew that Pearl Harbor had been attacked, about the handling of supplies on Bataan, about the retention of Richard Sutherland as Chief of Staff when MacArthur knew that Sutherland's abrasiveness made for difficulties with the Navy and the army commanders, about MacArthur's claiming credit for the island-hopping concept when others had demonstrated that strategy first, and about his dubious handling of public relations. James stresses that MacArthur's actions often had a touch of insubordination that he would not have tolerated in his own subordinates. James notes that we should not have been surprised by MacArthur's challenge to the President in Korea.

This is a perceptive account of a brilliant but erratic leader who failed to see the world conflict in a global setting and who judged strategy on the basis of the opportunities afforded him. The role of occupation chief in Japan would give him the chance he craved to be master in his own house.

FORREST C. POGUE  
*Eisenhower Institute for Historical Research*

LENORE FINE and JESSE A. REMINGTON. *The Corps of Engineers: Construction in the United States*. (United States Army in World War II: The Technical Services.) Washington: Office of the Chief of Military History, United States Army. 1972. Pp. xviii, 747. \$8.00.

In any complex organization each part tends to insist that its contribution is central to the successful functioning of the whole. Fine and Remington make an effective case that in World War II domestic, military-centered construction was the *sine qua non* of successful mobilization, armament, air supremacy, and the development of the atomic bomb. Responsible for \$15.3 billion worth of construction, the Army supervised the largest building program in our history with boldness and imagination.

Moving with precision through some necessarily unexciting matters, the book studies adequately land-acquisition policies, contract forms, congressional relations, labor disputes, and dealings with contractors large and small. Although the book is included in the sub-series on the Corps of Engineers, half of it is concerned with the Quartermaster Corps, which, until December 1941, controlled the largest share of the program. There is extensive and evenhanded coverage of the bitter,

three-way struggle among the partisans of the Quartermaster Corps, the Corps of Engineers, and those who supported a completely separate construction division. The Engineers were victorious and, the authors contend, clearly demonstrated superior capabilities for planning and construction. Never completely explained is the lack of adequate preparation by the Quartermaster Corps for national mobilization.

The authors have mined the extensive military records and interviewed or corresponded with several hundred participants in the gigantic effort. The work is technically precise, and the footnotes are where they belong. The writing style, while clear and steady, is occasionally marred by clichés. It may seem a minor point, but it is unfortunate that the authors insist on referring to the relocation of "west-coast Japanese," without noting that a majority of them were, in fact, American citizens of Japanese ancestry.

The final chapter on the Manhattan Project is clearly the most dramatic. The two-billion dollar leap into the scientific darkness linked the Army with industry, labor, and the academic community and provided the capstone of the Engineer's efforts in World War II. It is sufficient praise to conclude that this lengthy volume hews to the high standards of scholarship set by the earlier works in the series.

CHARLES W. JOHNSON  
*University of Tennessee*

JEAN EDWARD SMITH, editor. *The Papers of General Lucius D. Clay: Germany 1945-1949*. Volumes 1 and 2. Bloomington: Indiana University Press. 1975. Pp. xii, 524; ix, 527-1210. \$35.00 the set.

The publication of the recently declassified papers of General Lucius Clay should touch off debate within scholarly circles. On the basis of these volumes, the stereotyped view of General Clay as the originator of the Berlin airlift and dogged defender of American interests in Germany will give way to a more realistic appraisal of the man and his actions. The picture that emerges of Clay is that of a decisive, competent, and moral soldier increasingly disturbed by the breakdown of quadripartite government in occupied Germany. French intransigence, British suspicion, and growing hostility between the Americans and the Soviets brought about the demise of Allied cooperation and the division of Germany.

The editor of these volumes, Jean Edward Smith, judiciously selected 746 documents from Clay's term as military governor of Germany and presents them in chronological order with brief explanatory notes. The selection scans the wide variety of problems Clay dealt with, ranging from

the preservation of German art to the threat of war, but focuses on the key issues of occupational policies—recovery and the division of Germany. Complicated questions of currency reform, banking, decartelization, and dismantling reveal Clay's mastery of the art of governing. But even more impressive is the amount of influence he had in developing American policy in Germany. In contrast to the trend toward one-man, "star" diplomacy, Clay was more than just an executor of foreign policy—he helped shape it. His papers indicate he had as many disagreements with Washington as he did with Paris, London, and Moscow, but with unflinching determination he backed his arguments with facts, opinions, and, if need be, threat of resignation. More often than not, he won his case. For example, Secretary of State James Byrnes accepted virtually all of Clay's views on Germany in his famous Stuttgart Address of September 6, 1946. Clay was to have far less influence, however, on Byrnes' successors.

Clay's image as a cold warrior is not substantiated by these volumes. By late 1947, when Washington had all but given up trying to cooperate with the Soviets, Clay was still urging cautious but continual negotiations. Moreover, it was Clay who correctly estimated Soviet intent and advised standing firm on the airlift during the Berlin blockade.

Since the focus of the volumes is on the formulation and execution of policy, there is very little on the effects on Germany of U.S. occupation. But that is a minor flaw in what will be one of the major historical collections on the period. The editor and publisher deserve the appreciation of the academic world for their presentation of the Clay papers.

EDWARD L. HOMZE  
*Naval War College*

*Foreign Relations of the United States, 1948*. Volume 1, part 1, *General; The United Nations*. (Department of State Publication 8805.) Washington: Government Printing Office. 1975. Pp. xvi, 505, xv. \$8.10.

*Foreign Relations of the United States, 1949*. Volume 2, *The United Nations; The Western Hemisphere*. (Department of State Publication 8789.) Washington: Government Printing Office. 1975. Pp. xii, 827. \$10.40.

These documents reveal a dichotomy in American thinking about the UN's role. Major policy statements by Truman and other administration leaders of unequivocal support for the UN were not matched by actions at the international organization. America's reliance on the UN to resolve crucial issues in international relations in 1948 and 1949 was largely peripheral. Ambassador Warren

Austin and his colleagues devoted much energy to bringing the organization into the mainstream of handling major world issues.

Three areas illustrate the frustrations of their efforts. In 1948 there was concern over the Soviet Union's tactics which had blocked agreement in the United Nations Atomic Energy Commission (UNAEC). The U.S. had persuaded a majority to favor its Baruch Plan of 1946 and worried that dropping further efforts to reach agreement in UNAEC would enable Russia to charge the U.S. with halting negotiations as a rationale for an "atomic alliance." More important, most U.S. officials wished to keep the door open, hoping the Soviets would alter their policy. They also saw advantage in utilizing UNAEC for propaganda—to show the world the Russian refusal to accept effective controls of atomic materials. It is worth noting that throughout the extensive documentation on this subject, the Americans connected great power relationships to the atomic weapons topic and nowhere did they consider employing the U.S. atomic advantage to extract diplomatic concessions from the Soviets.

There was also frustration in the efforts to limit the veto power. The General Assembly's Interim Committee recommended a broad category of decisions where the veto would not apply, most notably in resolution of disputes by pacific means and on admitting nations to membership. Russia denied the legitimacy of the Interim Committee's work, and so all efforts in this field failed. This subject especially concerned State Department officials because public and congressional frustration was rising over apparent weaknesses of the UN. The U.S. delegation finally decided that the chief benefit in the issue was to bring public pressure to bear on Russia, showing the world how the Kremlin had barred progress toward reconciling disputes.

Russia's attack on NATO in 1949 as an aggressive instrument consumed much American attention. American attitudes had now hardened, and the documents indicate U.S. officials were pleased that the Soviets had moved openly toward "world domination." Otherwise, as Assistant Secretary for International Organization John D. Hickerson put it: "Had they lulled us into a sense of false security for a few years, they could easily have ruined us." U.S. spokesmen were eager to employ the UN as a vehicle for pinning the blame for the cold war on Russia. They devoted much of their time to marshalling support for "The Essentials for Peace" resolution to counter the Soviet denunciation of NATO, and the measure passed in November.

Other topics include relations between the U.S. government and UN headquarters. They range

from taxation of American employees, through delineating the area in which UN employees and foreign representatives would enjoy diplomatic immunity, to the problem of *laissez passer*. The latter question caused the greatest worry. State Department officials already felt the early waves of the anticommunist hysteria which were soon to sweep across America. Charges were made, some from within the department itself, that communist agents were gaining entry into the country via the UN for purposes of subversion. Material is disappointingly sparse on the important topic of the organizational relationship between the Department of State and the UN delegation. A few documents in the 1949 volume illustrate the lack of coordination on foreign-policy formulation, but historians will have to examine the files to pursue this subject.

Coverage of Western Hemisphere relations varies considerably in detail. Material relating to Canada is very scanty, confined to brief references on trade and mutual defense questions. Of the South American countries, strongest coverage is provided for affairs in Argentina, Bolivia, Brazil, and Chile. The volume indicates that neighbors of Argentina feared her intentions, especially during the revolutionary upheavals in Bolivia during 1949. U.S. representatives emerged in a mediator role. A significant amount of material on Latin America details economic foreign policy. The evidence suggests lack of a coordinated U.S. policy for the region, other than financial support through the Export-Import Bank or World Bank and modest military programs under the Mutual Defense Assistance Act. A joint Brazil-U.S. Technical Mission is covered, and the documents show American perception of the need to foster land reform to avert dangerous social revolution. The general tenor of U.S. officials reporting to Washington, however, was to stress immediate matters of political stability. With the exception of Colombia, Guatemala, and Panama, the theme of communist penetration is not pronounced in dispatches to Washington, although this problem did occupy Acheson's attention.

The editors of future volumes might consider providing fuller editorial summations of foreign-policy developments. Those that occur in these volumes are extremely helpful.

THOMAS M. CAMPBELL  
Florida State University

*Foreign Relations of the United States, 1949. Volume 3, Council of Foreign Ministers; Germany and Austria.* (Department of State Publication 8752). Washington: Government Printing Office. 1974. Pp. xxv, 1324. \$14.55.



Like most of its forerunners, this volume is indispensable to the study of American diplomatic history. In both formal and informal relationships the machinery of diplomacy is laid bare. At times this makes the reading of documents and editorial notes somewhat tiresome, but it compensates with extremely engrossing material, and at other times the book is difficult to put down. The documents' original classifications—"secret," "top secret," "top-secret routine," "top-secret priority," "top secret and personal," "top secret, no distribution," "eyes only"—do help in setting a value on subject matter, as the statesmen and diplomats who worked with them thought they should.

That some of the documents in this volume are captivating goes without saying, especially when the reader reaches the Berlin blockade. The Joint Chiefs of Staff took the position that if the Soviet Union reimposed the blockade of Berlin the United States might want to consider it "dangerously close to an act of war" (p. 819). Secretary of State Acheson responded that in the event "we should immediately inform Soviet Government that we consider it hostile act," and "we would be squarely faced with the issue whether we would break the blockade even at the risk of war" (pp. 826-827).

Composed of nearly 1,300 documentary pages, this "fat" book (State Department publication 8752) covers the establishment of the German Federal Republic at Bonn and its economic, political, and international problems, the Soviet Union's steps toward the formation of the so-called German Democratic Republic, and the American attitude toward the two Germanies, the blockade of Berlin, and the establishment of an independent Austria.

Since relations with Austria never played the same role in the popular mind as German affairs, the Austrian documents are extremely interesting: what the Allies had in mind, in what ways they differed among themselves, and how the Soviet Union might, some thought, set up a separate government in eastern Austria in order to give the Russians an important central European strategic center, including the Vienna transportation network and the Austrian Danube. The American legation in Vienna held that the advantage in having American troops in that city, even as a token force, was not that they could successfully resist a Russian absorption of the capital, but that as long as such troops were present any Soviet action *would also be against the United States*, involving "a threat of war."

The more dangerous locale was Germany, however, not Austria, even though Austrian policy turned on the objectives of terminating the Allied military occupation and restoring Austria's sover-

eignty, which the Soviet Union would not accept in 1949. Instead the Soviet Union gave strong support to Yugoslav claims on Austria's frontiers, including an autonomous government for Slovene-speaking people, in furtherance of Soviet objectives in Yugoslavia itself. President Truman said in October 1949 that, regardless of the Soviet Union's position, "we should adhere firmly to our position that we would not agree to an Austrian Treaty which made it impossible for Austria to survive as an independent nation." Nor did he believe paying \$200,000,000 a year to remain in Austria was an excessive price for preventing the Russians "from extending the Iron Curtain to the western boundaries of Austria, outflanking Germany and Yugoslavia, and positioning themselves at the Brenner pass" (p. 1168).

Still, there was no grave development in Austria and no war danger, as there appeared to be on the German-Berlin question. And on that, the documents show how the Western nations' agreement to a North Atlantic defense treaty resulted in the Soviet Union's abandoning measures of duress against the Western powers in Berlin.

How the Soviet Union refused to agree to a common German currency as a means of stalling on German unification and creating her own dependent East Germany makes extremely interesting reading even for people accustomed to the techniques of diplomatic aims and purposes. The unfolding of West Germany's metamorphosis from a subjected and conquered country into a trizonal fusion, to be followed by the adoption of the "Basic Law" for a sovereign West Germany is still something that holds one's attention in both general outline and detail—especially the detail, so much of which was not known before this volume was made available.

ALBERT NORMAN  
Norwich University

CHARLES C. ALEXANDER. *Holding the Line: The Eisenhower Era, 1952-1961*. (America since World War II.) Bloomington: Indiana University Press. 1975. Pp. xviii, 326. \$12.50.

JIM F. HEATH. *Decade of Disillusionment: The Kennedy-Johnson Years*. (America since World War II.) Bloomington: Indiana University Press. 1975. Pp. xvi, 332. \$12.50.

The appearance of volumes by Charles C. Alexander and Jim F. Heath constitute an impressive debut for the "America Since World War II" series under the general editorship of Warren Kimball. Together, they survey the cold-war years of Dwight D. Eisenhower through the demise of Lyndon B. Johnson's consensus. Developing the



themes of the most recent Eisenhower literature. Alexander weaves a deft analysis of the political, cultural, and economic conditions during the allegedly placid fifties, while Heath effectively reviews the turbulence that destroyed all hopes for a Democratic victory in 1968. Utilizing published sources almost exclusively and aiming mainly at the student, both are written with clarity, balance, and perception. The addition of comprehensive bibliographic essays further enhance works that ought to serve as models of their genre.

Neither Alexander nor Heath sees angels, devils, or dark conspiracies emanating from the White House. Both find individuals trapped by their own perceptions as well as the political and military realities of the United States in the cold-war. All three presidents reacted within the limitations of what they considered possible and necessary. Thus Eisenhower was not unlike most fellow Americans in seeing "the flow of world events through a prism of cold war conflict, of incessant struggle against a multi-faceted, nearly omnipresent Communist enemy" (p. 27). Heath's portrait of the Kennedy-Johnson period reveals two presidents who, representing a party that had been under persistent attack for being somewhat less than totally committed to "victory," were obsessed with "standing up" to communism. Caught between domestic forces that were perhaps beyond the scope of their ability to deal with, the ultimate result was failure. All that came despite the fact that, as Heath concludes, "Kennedy and Johnson were fundamentally decent men, whose ambitions both reflected and influenced the American people. The two Presidents believed that they could shape the destinies of the United States and the world by a combination of will and pragmatic knowledge" (p. 303).

Although judged as "confirmed a cold warrior as anyone," Eisenhower nevertheless emerges as the most effective of the three men. What William Shannon once termed the "great postponement" begins to "look more and more like a good job of holding the line—against the agonies and excesses of the post-Eisenhower years" (p. 292). Alexander believes that the president "revealed more understanding of the transformations the cold war had wrought in American government and economy than any other President. The commonly held belief that Ike knew more about matters involving the military than anybody else had much truth in it" (pp. 290-91). Undoubtedly, Eisenhower bequeathed neglect that inevitably plagued his successors, but Heath views Kennedy and Johnson as obsessed by the need to prove their strength. While much of what happened was beyond their control, the results were "often of their own making." By 1968, disarray, violence, and dissent characterized

America. "Instead of finding its Utopia, America became a country struggling desperately to escape its Armageddon" (p. 13).

Both historians offer some questionable judgments. Alexander contends that there *was* somewhat of a missile gap from 1959 until 1961. Heath ignores the surveys showing that Kennedy lost votes because of his religion and that the Catholic electorate also responded to economic conditions and concludes that a Protestant might not have done as well. Yet he offers no contrary evidence. Lamenting the plight of the civil-rights bill at Kennedy's death, Heath overlooks its extrication from the House Judiciary Committee in October and subsequent presentation before the full house on November 20, two days before the Dallas tragedy. These are minor quibbles, however, hardly enough to diminish enthusiasm for the forthcoming contributions to this series.

HERBERT S. PARMET  
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City University of New York

ROGER MORGAN. *The United States and West Germany, 1945-1973: A Study in Alliance Politics*. London: Oxford University Press, for the Royal Institute of International Affairs and the Harvard Center for International Affairs. 1974. Pp. x. 282. \$22.50.

Roger Morgan has the credentials for producing an overview of American-West German relations for the past thirty years: he is Deputy Director of Studies for the Royal Institute of International Affairs; he has written on contemporary German politics; and he spent a year at the Harvard University Center for International Affairs. Surprisingly, the resulting book does not reach the depth it could have achieved.

The fault stems mainly from the author's decision to employ a methodological scheme which is imposed upon, rather than derived from, the historical data. The structure of each of the ten narrative chapters is made to conform to a set mold: internal politics of both countries, bilateral relations, alliance politics, and relations with the adversary. With this "rigorous framework of concepts" Morgan hopes to furnish a paradigmatic tool for use on other bilateral alliances (pp. 3-5).

The method's shortcomings, however, as revealed in this experiment, caution against repetition. Some important subjects are merely sketched, while others, like Eisenhower's "tripwire" proposals, are ignored in order to make room for relative trivia. The very term "alliance" becomes ambiguous, meaning sometimes a bilateral "special relationship" and sometimes the larger NATO context. Factual errors accumulate, such as a reversal in referring to currency revaluation and

devaluation (p. 250), thus materially affecting the quality of the volume. Finally, the major conclusion of the study provides little that is new—relations between the two partners generally remained harmonious, but were determined at any given moment by the degree of compatibility in their stances toward the common adversary (pp. 247–48).

Yet the assessment should not be entirely negative. Although restricted to the Dulles Collection at Princeton for archival sources, Morgan has culled a variety of materials in English and German; therefore, the bibliography is good. Future writers should consult it long before their visit to the National Archives and presidential libraries, which have begun, with agonizing slowness, to release the documentation from the American side needed to survey meaningfully postwar relations between the United States and its best continental ally.

JOSEPH MAY

*Youngstown State University*

LYNN ETHERIDGE DAVIS. *The Cold War Begins: Soviet-American Conflict over Eastern Europe*. (Written under the auspices of the Institute of War and Peace Studies, Columbia University.) Princeton: Princeton University Press. 1974. Pp. x, 427. \$15.00.

Analysts of the Cold War have generally found the origins of that conflict in three disputed areas of Soviet-American relations—America's use of the atomic bomb, U.S. economic policy toward Russia, and the confrontation over Soviet demands for influence in Eastern Europe. Lynn Davis focuses upon American policy as it related to the last issue, from the first stirrings of U.S. concern about applying the principles of the 1941 Atlantic Charter to the final reluctant, resentful acceptance of Soviet hegemony in this area. Davis' study was spurred, she notes, by "the simple question of who is right," the consensus or the revisionist historian, in the Cold War debate on Eastern Europe. That question remains, inevitably, unresolved by her study, which sides most often with the consensus viewpoint. But what disappoints most is how little the book adds to this debate. The first scholar to produce a book-length account of American policy toward Eastern Europe, using the extensive and recently declassified files of the State Department, Davis nonetheless offers little more than an elaboration of the familiar argument that America followed a naive "policy of principle" in attempting to get Russian acceptance of self-determination for Poland, Bulgaria, Romania, and Hungary. Not realizing that Soviet and American aims would conflict at war's end, she argues, American planners decided to defer talks with the Russians until victory over Germany had been won. What the

author sees as a U.S. "policy of postponement" concerning Eastern Europe is, ironically, in line with the revisionist critique—but with the vital difference that Davis ascribes this strategy of delay to a belief that "the Allies should fight now and negotiate later," and not to an attempt at stalling the confrontation with Russia while America's economic and atomic weapons were readied. Roosevelt's attitude at Yalta, she concludes, marked the abandonment of postponement—if not principle—in U.S. policy, with the result that Russia seemingly accepted U.S. ideas on self-determination at that conference. "Angry, shocked, and disillusioned" at later Soviet violations of the Yalta agreement, American planners (including Truman) continued Roosevelt's insistence upon the principles of the Atlantic Charter, even as it became clear that the U.S. had neither the vital interest nor the military means to prevent Soviet dominance in Eastern Europe.

Unfortunately, Davis' book passes by some of the most interesting and important questions of the Cold War, while dealing with major controversies in only a cursory fashion. This flaw reflects Davis' concern with policy but not the assumptions behind it. This also caused the author to ignore valuable evidence. The result is not only that Davis' book fails to break new ground in the interpretations of Cold War origins, but that many of her familiar conclusions remain suspect.

Chief among these is Davis' central contention that principle was the guiding motive behind American policy; here she fails to deal with the revisionist charge that—as with the policy of the Open Door—"principle" was but a disguise for self-interest without power. The author, curiously, even seems to suggest the same when she lists as last among five considerations directing State Department planners on Eastern Europe, that of a "strong personal commitment to implementation of the Atlantic Charter principles"—behind "ideas about how peace should be constructed after the war." Typical also of the limitations of her analysis is the assumption that atomic diplomacy meant only the explicit threat of the bomb's use—a narrow definition which caused her to miss the importance of the weapon in discussions of Eastern Europe at the post-war meetings of foreign ministers (and, apparently, to ignore in her research the papers of Secretary of State Byrnes on that subject).

Though Davis' study is definitive neither on the question of how the Cold War began nor on the place of U.S. policy toward Eastern Europe in its beginnings, her book is a start toward a scholarly answer to the subject. It has historiographical interest, as well, as a reaction not to

the Cold War issues directly but to the controversy that still surrounds them.

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ABRAM CHAYES. *The Cuban Missile Crisis: International Crises and the Role of Law*. Published under the auspices of the American Society of International Law. New York: Oxford University Press. 1974. Pp. viii, 157. Cloth \$5.95, paper \$1.95.

This is the first volume in a projected series, "International Crises and the Role of Law," to be published under the auspices of the American Society of International Law. Abram Chayes, now at Harvard Law School, was legal advisor to the State Department during the Cuban missile crisis, and he has been concerned with the subject ever since. The study, financed by the Carnegie Corporation and the Old Dominion Foundation (Mellon), adds little new except a 1968 letter from N. A. Schlei on Justice Department activities in August 1962. For the facts, historians must still turn to Elie Abel (1966), Graham Allison's *Essence of Decision* (1971), and the memoirs of participants.

Chayes examines the role of law in three aspects of the crisis: the use of blockade rather than alternative actions; the appeal to the Organization of American States; and the approach to the United Nations. He decides that the role of law was "substantial." The process by which he reaches this conclusion is more revealing of the corruption of contemporary legal thinking than of the events of October 1962. All three aspects of the crisis were political, not legal, but Chayes smuggles in a legal element by blurring distinctions through verbal ambiguities. He rejects the sharp distinction made by the participants between policy questions and legal questions on the grounds that power is reflected in both. He rejects William P. Gerberding's suggestion that legal analysis is rationalization, "something cooked up after the event," and he rejects Robert F. Kennedy's statement that the OAS vote was political, not legal, by phrasing his discussion of this vote in terms of "authorization" and "justification." The use of "authorization," a legal term, and of "justification" rather than "rationalization" helps disguise the fact that the blockade was a political act in an area (Caribbean) and with a weapon (the Navy) where the Soviet Union could not resist except by using a different weapon (strategic) or area (Europe) which neither side wanted. Calling the blockade a "quarantine" was part of the deception since international law insists that a blockade is a political act, not a legal one. Chayes' conclusion that "'mere' justification carries greater practical im-

portance for the success or failure of great decisions than is commonly supposed by the analysts" (p. 48) neither refutes Gerberding nor supports Chayes' principal argument.

Like any honest brief for a poor case, this volume is full of inconsistencies. Chapter III ends with the statement that Article 2(4) of the United Nations Charter "was a significant factor in determining the decision against air strike or invasion and for . . . a quarantine." Chapter IV then turns to the "authorization" by the OAS, although if a "quarantine" was legal there was no need for any "authorization," and the appeal was political not legal.

Those who think this volume adds little to history will find it more valuable for evidence on the sociology of law at the highest levels of our society today.

CARROLL QUIGLEY  
*Georgetown University*

EDWIN BICKFORD HOOPER. *Mobility, Support, Endurance: A Story of Naval Operational Logistics in the Vietnam War, 1965-1968*. Washington: Naval History Division, Department of the Navy. 1972. Pp. xviii, 278. \$4.25.

RICHARD G. HEWLETT and FRANCIS DUNCAN. *Nuclear Navy, 1946-1962*. Chicago: University of Chicago Press. 1974. Pp. xv, 477. \$12.50.

The end of World War II found the United States Navy, which had established control of the seas to an extent never before approximated, faced with the task of defining its role as an instrument of national policy. It was a crucial time when the nation was assuming broader world-wide responsibilities, and advances in technology promised to render obsolescent the weapons that had produced victory. These books reveal two ways the navy responded to the challenge and the demands made on the service. Both are written by members of the government who had access to materials that will long be denied other historians.

Admiral Hooper presents a first-person narrative derived from his experience as commander of the Service Force of the U.S. Pacific Fleet, which furnished the supplies that made it possible to sustain military operations in Vietnam. Providing logistical support for the waging of limited war in distant parts of the world was a role few in the navy had visualized, but for which many were prepared by having mastered the enormous supply problems in the Pacific during the Second World War and the Korean War. This account of a less-publicized but no less vital dimension of the Vietnam involvement possesses the advantages and shortcomings of an eyewitness account by one who

views the events with a detached perspective often lacking in autobiography.

The authors of *Nuclear Navy, 1946-1962*, both historians with the Atomic Energy Commission, have produced a detailed, heavily documented study of what amounted to one man's prevailing against the system. Although there had been an awareness at the top of the navy hierarchy of the implications of nuclear propulsion for warships, there was little awareness of how this should be accomplished. The selection of Hyman Rickover and the ways in which he overcame seemingly insuperable obstacles at virtually every stage constitute most of the story. Battling the navy, civilian firms, the Atomic Energy Commission, Congress, and almost everyone else involved, Rickover managed to supervise the development of practicable nuclear-propulsion systems, influence ship design, control the selection and training of crews, and intrude into command operations. Neither details nor personalities are lacking, with Rickover's single-minded dedication as a constant in the effort that saw thirty reactor-powered vessels at sea in 1962. The emphasis throughout is on technology, bureaucracy, and administration rather than on the strategic and tactical implications of nuclear-propelled warships.

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*University of Miami*

#### CANADA

LOUISE DECHÊNE. *Habitants et marchands de Montréal au XVII<sup>e</sup> siècle. (Civilisations et mentalités.)* Montreal: Plon. 1974. Pp. 588.

Louise Dechêne's work has already received one significant accolade: the Governor-General's Prize for History, French-language section, despite the fact that her thesis was written under the direction of Robert Mandrou of the Sorbonne and was published in France rather than in Quebec. Evidently, Québécois cultural colonialism is far from dead.

Another facet of this colonial mentality is the slavish adherence to current French forms of historical methods, producing in effect, a methodological colonialism. The author, with less rather than more success, has tried to apply to the analysis of a new society, specifically the establishment and development of Montreal in the seventeenth century, the sophisticated perceptions characteristic of Mandrou's work on prerevolutionary France. The series in which Dechêne's work appears is entitled "Civilisations et mentalités," surely an odd series for the history of a settlement in the wilds of North America. Dechêne also uses French monetary values rather than colonial ones as well as kilometers instead of the usual leagues.

The anacronisms expose the basic weakness of the work: the book is concerned with the society of Montreal looked at from France in the twentieth century rather than from the colonial society itself in the seventeenth and eighteenth centuries.

One presumes that the author's original intent was to study the "mentalities" of the social components of early Montreal along the lines developed by her thesis master, Mandrou. If this was Dechêne's purpose, then the ends have not been achieved. There are nevertheless a vast number of data in the book, much new information, and valuable and valid insights into the parts of a nascent colonial society. One reads the almost 600 pages, however, and asks oneself: what has been accomplished?

CAMERON NISH  
*Concordia University,  
Montreal*

RICHARD ARTHUR PRESTON, editor. *For Friends at Home: A Scottish Emigrant's Letters from Canada, California and the Cariboo, 1844-1864*. Montreal: McGill-Queen's University Press. 1974. Pp. xii, 338. \$12.00.

James Thomson (1823-95), a literate Aberdeenshire baker's apprentice, wrote the letters printed here, in most cases, to his Scottish relatives. Diaries of his travels, also included in the book, fill out the story. Thomson migrated to Montreal in 1844, where he worked at his trade for a few months before moving to southeastern Upper Canada (Ontario). There, he baked for Irish cannallers on the St. Lawrence River. In 1849, Thomson found himself in Chicago, on his way to the California gold fields where he arrived in 1850. After a visit to Scotland in 1853, he returned to Upper Canada. His only other travels took him on a misguided venture, via the Isthmus of Panama, to the Cariboo in 1862. On returning to his Upper Canadian home he successfully established himself as a farmer, a bookkeeper in a starch factory, and an elected municipal officer.

The documents give the views of a workingman and not the impressions of the usual descriptive writer. Thomson was an abstemious and devout adherent of the Free Church of Scotland and the cheering effect of his religious convictions runs through the pages. He wrote about things of which he knew and which interested him: methods and costs of travel, costs of food and equipment, details of the life of a steerage passenger, wages, and conditions of work. The documents, with Preston's introduction and editorial notes, were well worth printing.

JAMES J. TALMAN  
*The University of Western Ontario*

ROBERT E. CAIL. *Land, Man, and the Law: The Disposal of Crown Lands in British Columbia, 1871-1913*. Vancouver: University of British Columbia Press. 1974. Pp. xv, 333.

*Land, Man, and the Law*, a study of government land policy in British Columbia, will be of interest to historians of public-lands policy in North America. Its section on Indian lands should evoke a broader interest.

Cail begins with a short chapter on land policy in the period before British Columbia's 1871 union with Canada. Governor James Douglas emerges as the wise legislator, hampered only by practical circumstances and a parsimonious Colonial Office. Subsequent chapters trace the story of governmental provisions for alienating agricultural and pastoral land to 1913 and are followed by chapters on surveys, mining and timber legislation, and water rights. A central section deals with the complications which arose from the "railway belt"—the land turned over to the federal government to further the Pacific railway—and the provincial government's own grants to railway companies. A final section treats government policy toward Indian land claims and reserves, still a sensitive and unresolved question. A short conclusion separates the text from excessive appendices of documents and tables.

A good thesis does not make a good book. An old thesis tends to make a bad one. Originally written in 1955 for a M.A. degree, *Land, Man, and the Law* could not be maturely revised because its author was killed in a 1958 accident. The book has many of the features which characterize a thesis—and many of the faults. It is marred by excessive legal description, by a lack of comparative perspective, and by an unconcern with politics or even administration. The author consulted only published sources. Some sections have been superseded by later research and like most theses, it makes dull reading. The book is, however, a useful synopsis of aspects of settlement, resource, and Indian land policy in British Columbia.

DOUGLAS COLE  
*Simon Fraser University*

IRVING MARTIN ABELLA. *Nationalism, Communism, and Canadian Labour: The CIO, the Communist Party, and the Canadian Congress of Labour, 1935-1956*. Toronto: University of Toronto Press. 1973. Pp. vii, 256. Cloth \$15.00, paper \$4.50.

This timely work of scholarship makes skillful use of voluminous union records as well as of lengthy interviews with the leaders who played decisive roles in this seminal period for Canadian labor (1935-1956). Abella's detailed examination of the relations between Canadian unionists and their

American counterparts is a convincing rebuttal to earlier views that the CIO refrained from interfering with the Canadian Congress of Labor and the aspirations of more nationalistic Canada. "Right from the beginning the CIO was insistent upon showing the flag in Canada—the American flag, that is." Abella also shows how Canadians themselves, notably Charles Millard of the United Auto Workers, edged out both the more militant Canadian nationalists and the highly regarded and experienced Canadian communist labor leaders. Canadian workers seemingly cared little about foreign control of their unions so long as they achieved better working conditions.

At times I grew weary of the detailed descriptions of the interunion squabbles and yearned instead for more on the general economic picture. The study, after all, covers the last half of the depression and ends in the middle of the Cold War. Readers unfamiliar with modern Canadian history might wonder how and why leftist labor leaders gained and held such wide support in the face of persistent and ever-mounting pressure from the rest of Canadian society. This points to a different Canadian experience, one that becomes less distinctive as American unions followed their country's branch plants into Canada.

Minor cavels, perhaps, for what is an excellent study.

RICHARD WILBUR  
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Montreal*

DAVID R. MURRAY, editor. *Documents relatifs aux relations extérieures du Canada/Documents of Canadian External Relations*. Volume 7, part 1, 1939-1941. Ottawa: Department of External Affairs. 1974. Pp. cxcix, 1167. \$15.00.

The Canadian series of handsome volumes containing official documents on foreign policy has settled down to a standard format that is useful for scholars. Also valuable for general reference, it now provides the file numbers for the original documents; and the indexing, editing, organization, and introduction are convenient and helpful. This seventh volume, covering relations with Commonwealth members, allies, and neutrals and also various other war-related problems that are carefully grouped topically, excludes papers concerning relations with the United States. Nevertheless, as the size of the book shows, the war had brought at least a tenfold increase in the business of Canada's Department of External Affairs. Despite the deliberate omission of American matters, this volume, covering only two years, is larger than any of its predecessors.



The dominance of the American presence for Canada is such that despite the planned exclusion, United States influence intrudes at various places. In a section relating Canada's concern about the security of cryolite mines in Greenland after the Nazi occupation of Denmark, American suspicion that this was motivated by more than strategic interest brought a virtual veto. This incident, which has been written about by the official military historians in both countries, and also by James Eayrs in his *In Defence of Canada*, provides a classic example of the way in which jealous care for national interests can generate friction, and in which relative power decides issues even between friendly countries. In this case, as in many others, though Prime Minister Mackenzie King was determined at all costs to avoid upsetting American opinion, the State Department magnified one Canadian artillery officer in mufti into a military invasion. It showed no similar sensitivity about the implications of dispatching American troops. The documents presented here do not throw new light on the origin of the concern about Greenland, or whether the concern came from Britain or from the Aluminum Company of Canada. It is unlikely that the inclusion of more low-level policy papers from the Department, which some reviewers have demanded, would have cleared that matter up. Moreover, to go into that kind of detail would require larger volumes that would be prohibitively costly.

This volume demonstrates yet another Canadian neurosis in addition to hypersensitivity about the American relationship. The documents in the series are published in French or English, whichever was used in the original in the Department. There are few in French. Quite properly for a country that strives to stress its bilingualism, titles, headings, introductions, and indexes are in both languages, but translation of everything into English or French would be wasteful. In this volume, however, as in many Canadian federal usages, the French is always placed before the English. The adoption of diplomatic usage that followed alphabetical order in either language would have reversed that order. What seems to be a gesture of Francophilism in fact may impede the use of this tool as a reference since, even for persons fully conversant in French, additional headings in a second language in a superior position tend to impede rapid access.

RICHARD A. PRESTON  
Duke University

R. D. CUFF and J. L. GRANATSTEIN. *Canadian-American Relations in Wartime: From the Great*

*War to the Cold War*. Toronto: Hakkert. 1975. Pp. xiii, 205.

J. L. GRANATSTEIN. *Canada's War: The Politics of the Mackenzie King Government, 1939-1945*. New York: Oxford University Press. 1975. Pp. xi, 436. \$18.95.

JAMES EAYRS. *In Defence of Canada: Peacemaking and Deterrence*. (Studies in the Structure of Power: Decision-Making in Canada, Volume 6.) Toronto: University of Toronto Press. 1972. Pp. xiv, 448. \$17.50.

War has played a significant role in the evolution of Canadian-American relations. By war, I mean actual armed conflict between Great Britain (and therefore British North America, later Canada) and the United States in 1775 and in 1812, and the threat of armed conflict in 1838, 1861, 1866, and 1895. The two great wars of the present century, in which the former antagonists found themselves in alliance rather than in opposition, were even more important in determining the character of today's Canadian-American relationship. These two wars raised into bold relief the question of Canada's identity in relation to the United States and forced Canada to come to terms with its great neighbor in a way it had neither foreseen nor particularly desired. The late Harold Innis of the University of Toronto summed it up in the 1950s: "Canada moved from colony to nation to colony."

The three books under review deal with Canadian-American relations during and immediately after the Second World War. The least significant is the small volume by Cuff and Granatstein, the first a professor of American History and the second a professor of Canadian History at York University.

A collection of papers which have already appeared in various periodicals between 1969 and 1974, the book explores, although not in depth, various defense agreements between Canada and the United States—the Ordnance Department Agreement of 1917, the Hyde Park Declaration of 1941, the formation of NATO—and concludes with a warning of the dangers to Canada of seeking exemptions from American economic regulations in order to avoid the costs of national independence. The authors' conclusions are similar to those normally found among Canadian scholars of the nationalist left who argue that the principal threat to Canadian independence, since 1945, has come, not from the north but from south of the frontier.

Equally provocative and more lively is Granatstein's book on Mackenzie King, that enigmatic politician who, as Prime Minister of Canada, controlled the destiny of the country longer than any other party leader. Granatstein has not written a

biography; rather he has chosen to study certain aspects of the national effort from 1939 to 1945, examining the way in which King and the Liberal Government grappled with the basic political, financial, economic, and racial issues of the time. It has long been a popular academic sport in Canada to poke fun at King's foibles, to criticize his caution and to accuse him of failing to defend his country against encroachments from the south. It is clear enough, from a reading of Granatstein's book, that some of this criticism is justified; but it is also clear that on occasions King was more aware of the problems arising from Canada's close economic and political ties with the United States than were his colleagues. King's policies may not have been exciting or satisfying, but they were effective and successful. That is why, practically alone among wartime governments, his continued to enjoy public support after as well as during the Second World War.

*Peacemaking and Deterrence* is the third volume which, under the general title of *In Defence of Canada*, James Eayrs has contributed to the series "Studies in the Structure of Power: Decision-Making in Canada," sponsored by the Social Science Research Council. Eayrs' first two volumes attracted considerable attention, because they opened doors to historical sources closed to the academic community. This third volume is well up to the standard of the earlier works in terms of scholarship, mass of detail, and good, dull, academic prose.

Eayrs' book is a detailed study of Canadian government thought and action during the later years of the Second World War and the early years of the Cold War. In it the author, a professor of Political Economy at the University of Toronto, discusses the *dramatis personae*—the cabinet ministers, the civil servants, the generals, admirals, and air marshals—and the policies they espoused. He touches upon a multitude of topics, including the reorganization of the Canadian armed forces after the war, Canada's involvement with the United Nations, the Canadian role in the production of the atomic bomb, and the construction of the Mid-Canada and DEW radar lines. The book was scheduled to appear in 1966, but the stuffiness of a sensitive Department of External Affairs delayed publication until 1972. I do not think that Eayrs has much to complain about. The department has accorded him privileges other historians have not been granted, and his material is still as fresh as anything in his previous volumes. It is all here, with copious notes and references and lengthy excerpts from official documents—enormously useful to the student, but not very lively, for the point of view is pretty much that of the

department. Perhaps the officials of the department did know what was best for Canada, although some Canadians suspected that External Affairs was acting as if it were the Ottawa branch of the State Department.

GEORGE F. G. STANLEY  
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Sackville, N.B.

## LATIN AMERICA

ROBERT BRENT TOPLIN, edited with an introduction by. *Slavery and Race Relations in Latin America*. (Contributions in Afro-American and African Studies, number 17.) Westport, Conn.: Greenwood Press. 1974. Pp. xiv, 450. \$17.95.

The progeny from Frank Tannenbaum's seminal *Slave and Citizen* continue to be born. This collection of the hitherto unpublished results of original, often archival research into a number of Latin American societies is testimony to the rich materials available for the study of social history when the questions are sharply drawn. Editor Robert Toplin, who also has an original essay on the results of abolition for Brazilian blacks, briefly but often incisively introduces each of the fourteen essays, placing it in historiographical and comparative context. Like other recent works, these essays disagree almost unanimously with Tannenbaum. And even the single exception by Norman Meikeljohn, on the implementation of protective legislation for slaves in eighteenth-century New Granada, concedes that it was an uphill fight for crown officials to compel slaveowners to protect the slaves' humanity.

Not all Latin American societies are represented, though some are examined by more than one writer. (All but two of the authors are historians of Latin America.) Of major countries, neither Mexico nor Argentina is discussed, though Chile, the other South American society that "lost" its black population, is treated in an essay on Chilean blacks by William Sater, who emphasizes miscegenation in accounting for the disappearance. Only two Caribbean societies, Cuba and Puerto Rico, are discussed, in three essays that deal not only with slavery, but subsequent and contemporary race relations as well. The authors also make the point that the influence of the United States reinforced racial prejudice in Cuba and Puerto Rico, a point echoed in an essay on Venezuela.

Several countries have more than one essay devoted to them. Robert Conrad's piece on Brazilian slavery is a gem of interwoven fact and inter-

pretation, while Arthur F. Corwin's essay on modern Brazilian race relations is fresher and fuller than Florestan Fernandes' essay on a closely related subject. New Granada and Venezuela are discussed in two essays apiece, while Colombia is covered in a singleton by William Sharp, discussing the Chocó during the seventeenth century. Sharp's piece is valuable on the ease of manumission and the color discrimination against freedmen, but he becomes unnecessarily defensive about the paucity of slave revolts. John V. Lombardi, in writing on the abolition of slavery in Venezuela, also notes the absence of revolts over a period of some fifty years, but without any uneasiness. Thanks to Lombardi's piece and an excellent essay on racial attitudes in twentieth-century Venezuela by Winthrop R. Wright, this book expertly fills an important gap in the literature in English on slavery and race in South America.

The similarities in slavery and race relations north and south of the Rio Grande are pressed upon one by these new studies. Franklin Knight's essay notes, for example, that out of fear of slave revolts in the nineteenth century, Cuban authorities jailed visiting free Negro sailors, just as Alabama and South Carolina authorities did. Venezuela's constitutions have long prohibited the immigration of free blacks, a discrimination that even the United States has not practiced; Cuba's denominating Asian immigrant laborers as "whites" is less restrictive racially than United States policy, though no less anti-black. At the same time, the greater prevalence of miscegenation in Latin America and the absence of legal segregation certainly set apart Latin American race relations from those of the United States. Race prejudice is absent from no society in the western hemisphere, and as one essay points out, not even from Castro's Cuba. Yet the manifestations and virulence of prejudice against color certainly vary from society to society and even from period to period in the same society.

This book shows that the sharp contrast between North and South American race relations that was Tannebaum's contribution has now been transcended. Diversity in race relations among all New World societies within a general context of anti-black prejudice is the new and difficult challenge to historical and social explanation.

CARL N. DEGLER  
Stanford University

GABRIEL DEBIEN. *Les esclaves aux Antilles françaises (XVII<sup>e</sup>-XVIII<sup>e</sup> siècles)*. Basse-Terre: Société d'Histoire de la Guadeloupe; Fort-de-France: Société d'Histoire de la Martinique. 1974. Pp. 529.

Gabriel Debien's latest study on slaves in the French West Indies during the seventeenth and

eighteenth centuries makes a fitting climax to his long career of scholarly research based on plantation records. All his readers have been waiting for this book, and it will not disappoint his many admirers. Debien himself has almost disarmed criticism by very honestly pointing out in his introduction the main defects of the work, which are, in effect, the limitations of his essential sources—the plantation papers found in public and private archives.

These papers supply the basis for his research on eighteenth-century conditions among the slaves, especially at Saint-Domingue, where the documentation is richest. He has supplemented them with planters' guides, contemporary travel books, and other descriptive accounts. For the seventeenth century, he has not neglected the invaluable help of Dutertre and Labat, and he has also used those records of the administration which throw light on the state of the slave population in the Antilles.

From these sources, Debien has brought together a wealth of detail about the lives of the plantation slaves, ranging from their African origins, through their seasoning, to the various ranks of slaves and their cadres of management. He discusses the slaves' material conditions, their work, food, housing, and clothing, their health, high death rates, and low rates of natural reproduction, their religious state, their responses to slavery, including running away and other forms of resistance, their manumissions, and finally the question of whether their lot in life was ameliorated in the later eighteenth century.

Although most of these subjects have been discussed before, Debien has made a unique contribution by his tireless search through the plantation records. When he writes that about half of the new slaves died in the seasoning, we know that this conclusion results from careful statistical analysis. When he notes that underfeeding was the great shame of the colonial regime, and a great cause of running away, we know that this is what the plantation papers reveal. When he concludes that amelioration of slave conditions was dominated by considerations of revenue, we know that his findings are based on discriminating assessment of the evidence. While this is the great merit of all his work, it also accounts for some important omissions, particularly his failure to examine retention of African religious practices among the slaves. Where so much has been brought to light, however, what we now know with certainty in part compensates for Debien's characteristic reluctance to venture beyond those areas of research which his documents can readily illuminate.

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Jamaica

NICOLAS CHEETHAM. *New Spain: The Birth of Modern Mexico*. London: Victor Gollancz Ltd.; distributed by International Publications Service, Collings Inc., New York. 1974. Pp. 336. \$15.00.

This work, written by a British scholar and diplomat, is in this reviewer's opinion, the best synthesis of early Mexican history in English. The author has digested, synthesized, and organized most of the relevant secondary and some primary source material. He is familiar with and makes excellent use of both English- and Spanish-language material. Yet, despite the general excellence and readability of the book, he provides no new material for professional historians interested in the history of Mexico.

The work is divided into five parts. The sections include pre-Colombian nations, the conquest, the early period of Spanish rule under Cortés, viceregal government, and the attempts of the Spanish government and church authority to bring about a cultural, social, and political integration of the Indian subjects. The weakest chapters are the early ones on pre-Colombian civilization and the conquest. Much of this material has been covered in greater detail in other works. Reading Cheetham's book is no substitute for reading Bernal Díaz's account or Robert Padden's study of several years ago. The section on Spanish colonial government and the Spanish attempts at cultural integration contains much material that is otherwise unavailable to the lay reader. The author not only uses the most recent scholarly material to explain this period, but he uses it in a remarkably exciting way. His approach to the Aztec government and that of the Spanish colonial authority is even-handed and scholarly. His general interpretation is obviously influenced by Hanke, Gibson, and Ricard.

This is a very welcome and useful edition to the general literature on New Spain. I would recommend it to anyone going to Mexico or to anyone with a general interest in Mexican history, colonialism, or the anthropology of indigenous peoples. It would be very helpful for this work to be made available in paperback so that it could be used in general courses on Mexican and Latin American history.

ALBERT L. MICHAELS  
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Buffalo*

J. I. ISRAEL. *Race, Class and Politics in Colonial Mexico, 1610-1670*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xiii, 305. \$25.00.

We are gradually learning more about the seventeenth century in Mexican history, and with this

impressive contribution by J. I. Israel scholars have a major contribution in both information and interpretation. The terms race, class, and politics in the title convey only a partial idea of the book's intricate and intermeshing topics, which include ecclesiastical hierarchies, the seventeenth century's economic crises, illegal profit-making by viceroys, and many others. With respect to the title the dates 1621-1664 more accurately reflect the book's contents than 1610-1670.

Race and class are first analyzed in separate chapters on Indians, Spaniards, and others, with a valuable chapter on some minority groups that have been generally overlooked: Basques, Portuguese, Italians, and Jews. The author then moves to the section on political life, especially the upper-level political life of the viceregal capital. Here the controversies that so frequently disturbed the life of the colony, together with the rumors, threats, scandals, and related incidents, are recounted in detail. Not since H. H. Bancroft—who receives praise as a “great” historian—has this kind of material been taken so seriously. But what Israel does that Bancroft failed to do is to impose order and system on a history that has seemed to most students a miscellany of unrelated or superficial squabbles. Conflicts between viceroy and archbishop are shown to have an unexpected continuity over time, with the secular clergy, largely Creole, functioning as the viceroy's opposition. The tumult of 1624, which a generation of historians, following C. L. Guthrie, has attributed to shortages and high prices, is assigned here to political causes. The development of topics in the local Mexican scene is clarified, wherever possible, by reference to the particular circumstances of the parent country. Thus readers learn, for the first time so far as this reviewer is aware, of the connection between the Portuguese independence movement of 1640 and the rivalry in Mexico between the Duque de Escalona and Juan de Palafox y Mendoza. There are many other examples.

In recent decades the tendency among historians of Mexico has been toward social, socioeconomic, or even anthropological interpretations. Israel's is a socio-political interpretation, with emphasis on the political, and its revisionary implications are far-reaching.

CHARLES GIBSON  
*University of Michigan*

A. J. R. RUSSELL-WOOD, editor. *From Colony to Nation: Essays on the Independence of Brazil*. (The Johns Hopkins Symposia in Comparative History, volume 6.) Baltimore: The Johns Hopkins University Press. 1975. Pp. xi, 267. \$12.50.

The years 1750-1822 were of decisive importance for Portugal and Brazil, beginning with the ascendancy of the strong-minded reformer and fu-

ture Marquis of Pombal and ending with political separation. On the sesquicentenary of Brazilian independence an international symposium of scholars met at Johns Hopkins University to reassess the preconditions and meaning of Brazilian independence. Of the eight papers published here, three have already appeared elsewhere.

The stage is set with an overly long, somewhat repetitive introduction by A. J. R. Russell-Wood, well-known Brazilianist and organizer of the symposium, who analyzes the nature of "the colonial pact" between Brazilians and the metropolitan regime and provides a detailed survey of the socioeconomic development of late colonial Brazil. Emilia Viotti da Costa examines the breakdown of the colonial pact during the Court's thirteen-year residency in Brazil (1808–1821). As she perceptively demonstrates, Regent and later King John VI was torn between a desire to dismantle long-standing restrictions upon Brazilian economic activities and a need to safeguard traditional Portuguese interests in his nation's most valuable colony. Another Brazilian scholar, Maria Odilia Silva Dias, emphasizes that the independence movement was neither revolutionary nor nationalistic and did not produce a national consensus until mid-century. Stanley E. Hilton attacks the not very widely held "myth" that between 1808 and 1824 Brazil and the United States shared common ideals and values by demonstrating that the contrary was true.

The next pair of essays focus upon social themes. Stuart B. Schwartz contends that after 1750 the social structure of Brazil became increasingly complex, in part because of the growth of an independent peasantry, and that a major task of the Brazil-

ian empire after 1822 was to find a satisfactory formula "to accommodate the social tensions created in the late colonial era" (p. 134). In a characteristically graceful and informative paper Richard M. Morse tells us much about the urban development of Brazil from the sixteenth to the late nineteenth century but nothing about the communities' involvement in the imperial crisis of 1820–1822.

The last essays are concerned with intellectual topics. In his elegantly written study Manoel Cardozo analyzes the significance of the passing of the Baroque mood in Portugal and seemingly believes that Brazilian independence was hastened by the declining influence of the Church and the nobility and by unwise Luso-Brazilian acceptance of untried liberal dogmas. In an equally well-documented essay E. Bradford Burns assesses the influence of Brazilian urban intellectuals who were leading advocates of change. Well read though they may have been, it is difficult to see from Burns' discussion how such a group helped engineer Brazil's independence.

It is unfortunate that the editor did not indicate major areas of agreement and divergence among his authors or summarize ways by which our knowledge of the causes and consequences of Brazilian independence has been advanced by these essays. It is likewise regrettable that the essayists did not examine more closely internal developments within Portugal between 1807 and 1820 or the impact upon Brazil of revolutionary movements in Spanish America and in Europe. A useful but not vital volume.

DAURIL ALDEN  
*University of Washington*



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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

In her review of Fawn Brodie's, *Thomas Jefferson, an Intimate Portrait* (December 1975), Lois Banner confines herself to discussing the author's "highly speculative" psychological analysis. Opinions may vary on the merits of such speculation; but we should look also at the author's treatment of the facts of Jefferson's life. Facts can be judged objectively. Furthermore, speculation that disregards facts is hardly worthy of serious discussion. If her treatment of the period June–September 1776, with which I am familiar, is representative, Fawn Brodie's performance is appalling:

"We do not even know for certain if Jefferson signed [the Declaration] on the second of July, when the Declaration was formally voted, or on the fourth, as he later insisted" (p. 124). As Jefferson's Declaration of Independence was "formally voted" on the fourth, no one could have signed it on the second. Fawn Brodie has confused July 2, the day on which the Lee resolution passed, with August 2, the day the Declaration was signed. The uncertainty to which she refers is over whether the Declaration was signed on July 4, or only on August 2.

"But Jefferson had enough of anxiety. Without waiting to the end of the year, as he had earlier promised, he resigned his seat in the Congress on September 2, 1776 . . ." (p. 126). The year referred to is not the calendar year, but the year of Jefferson's term as a delegate. It expired in early August, and he planned to leave on the eleventh. Fawn Brodie thinks that Jefferson left Philadelphia four months early, when, in fact, he stayed an extra three weeks.

JAMES MUNVES  
New York, N.Y.

### FAWN BRODIE REPLIES:

The error pointed out by James Munves concerning the date the Declaration of Independence was signed has long since been corrected in my volume. The facts were originally written correctly, but in repeated typings and revisions the error crept in and was not, alas, caught before the first printing.

As for the second error, let me point out that Jefferson wrote to Edmund Pendleton June 30, 1776, "I shall with cheerfulness continue in duty here till the expiration of our year. . . ." Munves is correct in interpreting our year to mean "the year of Jefferson's term as delegate," not the calendar year. If I have made errors no more "appalling" than this I shall count myself more fortunate than most authors.

### TO THE EDITOR:

As coauthor of the U.S. Army's history of the China-Burma-India Theater of World War II, I am taken aback by some of the assertions and suggestions of Walter La Feber in his "Roosevelt, Churchill, and Indochina: 1942–45" (December 1975). If one may judge by his footnotes and text, he did not familiarize himself with the 1944 Chiang-Roosevelt correspondence, nor with the chronology and course of operations in China and

Burma that same year, nor yet with the full U.S. response to the 1945 Japanese seizure of Indo-China. For 1944, he has relied heavily on a December 4, 1944, memo from Roosevelt to an Admiral Brown. The results are not happy. La Feber writes (p. 1287):

By the summer of 1944 the president's Indochina plans were in danger. British pressure, French determination, and State Department opposition were building, *but the immediate threat arose from a crisis in China* [emphasis added]. During the spring, the Japanese attacked Stilwell's [sic] forces, gravely endangering the entire Allied position in South China. Roosevelt pleaded with Chiang to use the ten [sic] American-equipped divisions in Yunnan. . . . On April 3, *as the situation worsened* [emphasis added], Roosevelt again implored Chiang to move against the Japanese. The Chinese leader never acknowledged the note. As a White House analysis of this affair concluded [sic], "there was no solution to this impasse and the President stopped trying to prod Chiang Kai-shek into action."

One may note that the Japanese offensive in East China, ICHIGO, began April 17. It could not have affected FDR's April 3 message. FDR's pleas to Chiang to use his U.S. equipped Yunnan divisions in Burma actually began March 17, 1944. Roosevelt discussed the current Japanese offensives against the British IV and XXX Corps in Manipur State and XV Corps in the Arakan plus Stilwell's successes in North Burma. Roosevelt saw all this as opening the way for Chiang's Yunnan divisions to cross the Salween into Burma to help end the Japanese blockade of China. FDR's April 3 again urged Chiang to seize the opportunity FDR saw in Burma. Neither message discussed a crisis in China. As yet, there was none.

Though one may write that Chiang never acknowledged Roosevelt's April 3 radio, it is not correct to suggest he did not comply with FDR's request. On April 10, General George C. Marshall made explicit a hint in the April 3 message that there would be no more lend-lease for the Yunnan divisions if they did not attack into Burma. Stilwell's chief of staff so informed the Chinese. On April 14 the Chinese Army's chief of staff, Gen. Ho Ying-chin, agreed to attack. The Chinese made good on his promise May 11 and began the 1944-45 Salween Campaign. This act was Chiang's response to FDR's April 3 radio.

La Feber next deals with Roosevelt's attempt to persuade Chiang to let Stilwell command the armies of the Central Government and of the Communists. He writes (p. 1288): "In mid-summer Roosevelt asked that Stilwell be given control [sic] of China's armies in the war area [sic]. Chiang again refused to reply directly." FDR's request was delivered to Chiang on July 6. Chiang's immediate, direct reply was sent July 8. The full texts,

precisely cited, were published in 1956 in *Stilwell's Command Problems* (pp. 383, 385). Today the originals are in the National Record Center and the National Archives, respectively. The correspondence which followed is dealt with in *Stilwell's Command Problems*.

Finally, I cannot reconcile La Feber's suggestion on pages 1294-5 that "Roosevelt was finally defeated . . . by his own advisers and the Chinese crisis of 1944. On the Indochina issue he utterly failed as head of government, for he proved unable to impose his policy upon either the State Department or the military." This does not accord with the record. As I have indicated, La Feber does not grasp the course or the content of the China crisis of 1944. Regarding FDR's control of the military concerning Indochina I cite but one example.

Though La Feber describes Roosevelt in March 1945 as resisting "for several days" (p. 1293) French pleas for air support in Indochina, he shows no awareness that in April, a full month later, both the Joint Chiefs of Staff and the U.S. theater commander for China, Lt. Gen. A. C. Wedemeyer, were asking FDR if anyone, British or American, could give air support to the French (*Time Runs Out in CBI* [pp. 259-61]). This, after Roosevelt had allegedly surrendered to a military desire to support the French! And this request for orders from the men Roosevelt allegedly could not control! This exercise of civilian control must have been one of Roosevelt's last acts.

RILEY SUNDERLAND  
*Bar Harbor, Maine*

#### WALTER LA FEBER REPLIES:

I value the attention Riley Sunderland has given my essay, for like many other scholars I have admired and benefited from his past work. Of course I consulted both *Stilwell's Command Problems* and *Time Runs Out in CBI* before writing the essay, and I have seen the FDR-Chiang correspondence. Indeed, I examined most of that correspondence at the Roosevelt Library where the staff was good enough to declassify documents from the exchanges as I worked. On the other hand, as far as I can tell, Sunderland's volumes made little use of the FDR Library materials, relying largely on Roosevelt's messages to Chiang that were sent only through U.S. Army channels (*Time Runs Out in CBI*, [p. 16]). Sunderland's work could not exploit British Foreign Office documents, sources vital to my argument and essential to any work on Roosevelt or on general diplomacy and military strategy in these years. With these factors in mind, and having reconsidered the evidence, I see no reason to retract any part of my essay.

Riley Sunderland's detailed criticisms will be dealt with below. More important, we should be

clear that fundamental issues of American policy in 1944–45 are at stake in this exchange. The most important part of his attack, therefore, is in his final two paragraphs, not in the first paragraph he selected from my essay. (In fact, attacking this paragraph from my essay makes little sense, for it could have been reduced to the single irrefutable sentence, “by early autumn of 1944 FDR no longer had confidence that Chiang could act as the policeman for American interests in Asia,” and not one other word of my thesis would have had to be changed.) Sunderland apparently does not believe, for example, that the Chinese crisis was a major reason why Roosevelt had to ditch his plans for Indochina. Yet Sunderland cannot question my assertion that before 1944 the president looked to China as the policeman for Indochina. Nor can he question Roosevelt’s turn against Chiang after the 1944 Sino-American crisis and the concomitant turn in FDR’s Indochina policy.

In his final paragraphs Sunderland argues that Roosevelt did impose his Indochina policies on the U.S. military. Two points should be made. First, Sunderland’s last assertion (that the military chiefs asked FDR for orders in April) is irrelevant. I never claimed the chiefs did not, nor is the point interesting, for the crucial decision to aid the French was made in January and repeated in March, as noted in my essay. It would have been helpful if Sunderland had discussed Roosevelt’s decisions in January and March in *Time Runs Out in CBI* on pages 259–61; unfortunately, he did not.

Second, I know of no scholar who, after examining the evidence, has argued that Roosevelt imposed his trusteeship policy for Indochina on the U.S. military. I cited Secretary of War Stimson’s statement to Roosevelt on November 17, 1944, (again found in the FDR Library) to support my point. Scholars may argue, but I prefer the Secretary of War’s evidence in 1944 to Sunderland’s assertion of 1976. Moreover, and of primary importance, Sunderland cannot contradict my argument that the Joint Chiefs’ determination to gain sole control of Japanese-held bases in the Western Pacific after the war helped undercut Roosevelt’s hope for a meaningful trusteeship plan. If the Americans would not tolerate an international trusteeship agency overseeing these areas, Roosevelt could hardly have argued that the French should allow such an agency into Indochina. The British, among others, fully appreciated at the time the effect the U.S. military policy would have in weakening Roosevelt’s trusteeship plans.

Finally, Sunderland’s more detailed, if less important points can be considered. First: The December 4, 1944, memo from FDR to Admiral Brown on the Chinese situation is a crucial document, and one I could not find cited in Sunder-

land’s two volumes on this subject. A long account of the Sino-American crisis of 1944, the document is critical since it was drawn up by George Elsey and represented the White House view of the crisis. Roosevelt read it and wrote “excellent” on the report. My essay attempted to see Asia from FDR’s viewpoint; for this reason, the document is more important for the understanding of the president’s policies than histories which apparently do not use the document.

Second: The Japanese offensive referred to in my essay did not begin April 17 but with the attack on Imphal in March (see *Stilwell’s Command Problems*, [pp. 191 ff.]).

Third: The resulting crisis for Stilwell and the overall American position in CBI is well described in Barbara Tuchman, *Stilwell and the American Experience In China* (particularly pp. 439–43), in *Stilwell’s Command Problems* (pp. 174–75, 191 ff.), and especially in the Elsey memorandum of 1944.

Fourth: To claim that Chiang complied with Roosevelt’s plans simply by sending men across the Salween is a gross exaggeration. The thinness of these forces is described in Tuchman (p. 452). Roosevelt had much more in mind than this response by Chiang, and Elsey’s memorandum accurately characterized Chiang’s foot-dragging.

Fifth: Sunderland thinks Chiang’s reply of July 8 was a “direct” answer to FDR. This view, I fear, results from a misunderstanding of what I meant by “direct.” I meant that Chiang refused to tell FDR either yes or no. The reply was a masterpiece of prevarication; it held out the carrot of unspecified cooperation while sidestepping the entire issue by asking for “an influential personal representative.” Sunderland noted in 1955, when describing this exchange: “Later events suggest that the Generalissimo resolved that Stilwell should on no account hold command in China Theater, and that he rallied all his diplomatic resources to the task of avoiding any such outcome of the crisis” (*Stilwell’s Command Problems* [p. 385]). I find his 1955 views compelling.

In conclusion, therefore, I reaffirm the points of my essay: Roosevelt’s necessary change of policy on Indochina in late 1944 and early 1945, his growing break with Chiang as a cause of that change, and his failure to impose his original trusteeship idea on pivotal Washington officials. On these issues I find no reason to accept Riley Sunderland’s views.

It must further be suggested that if my interpretation of Roosevelt is to be challenged, at least use must be made of both the extensive sources in the Roosevelt Library (especially Elsey’s summary stressing the developing crisis in Sino-American relations before the Japanese offensive of April 17) and the British records of FDR’s conversations and actions.

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## Recent Deaths

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VERNER WINSLOW CRANE, emeritus professor of history in the University of Michigan, died at Ann Arbor on December 11, 1974, at the age of 85. Born at Tecumseh, Michigan, into a family devoted to scholarship—the late Ronald S. Crane was his older brother—he obtained his baccalaureate degree from the University of Michigan and was elected to Phi Beta Kappa. He was a graduate student at Harvard and at the University of Pennsylvania, from which he received his doctorate in 1915. Appointed as instructor in the following year at Michigan after postdoctoral study, he served in that institution during his entire teaching career except for a decade after 1920, when he joined the staff of Brown University. He returned to Ann Arbor in 1930 to succeed his mentor and friend, Claude H. Van Tyne. He was a visiting lecturer at Harvard, Brown, and University College, London. During the years 1928–34 he was a member of the editorial board of the *Review* and later served in a similar capacity for the *Mississippi Valley Historical Review*. He achieved eminence as a scholar with the publication of his pioneering study, *The Southern Frontier, 1670–1732* (1928).

Acquiring an international reputation as a specialist in colonial American history, Crane devoted more than four decades to research on Benjamin Franklin, publishing *Benjamin Franklin, Englishman and American* (1936), *Benjamin Franklin and a Rising People* (1954), and his edition of *Benjamin Franklin's Letters To The Press, 1758–1775* (1950), in which he added substantially to the Franklin canon by identifying pseudonymous writings. It is hoped that a manuscript concerning the circle of Franklin and his friends in London, which he had virtually completed before his death, will soon be published.

There was in Verner Crane an element of the careful antiquarian befitting a historian whose researches were concentrated in early American history—he was a corresponding member of the Colonial Society of Massachusetts and an honorary member of the Old Colony Historical Society, as well as an active member of the Board of Governors of the William L. Clements Library. But his interests were broad, his outlook generous. A most

meticulous scholar, he did not neglect the teaching of undergraduates. He excelled in the direction of graduate students. He was in his mature years a person of dignified appearance, and his piercing gaze—in part a product of his spectacles—was most disconcerting to students who had done less than their full duty. He was highly respected by his colleagues because of his maintenance of high standards and his devotion to principle, hence his appointment in 1938 as Henry Russel lecturer, the highest honor bestowed by the University of Michigan faculty upon one of its members. Not so well known was his unfailing kindness, especially to those in whom he placed trust, for he did not seek popularity. Always a gentleman, he possessed a lively sense of humor, and he dearly loved an amusing anecdote. He was gracious and witty in the last days of his final illness.

JOHN R. ALDEN  
*Duke University*

The death of CONSTANCE McLAUGHLIN GREEN on December 5, 1975, at the age of 78, takes from the historical profession one of its more delightfully colorful members. The daughter of Pulitzer Prize winning A. C. McLaughlin of the University of Chicago, Connie Green, as she was known to her large circle of friends, turned to history belatedly. After raising three children, she completed her doctorate under the direction of Ralph Gabriel at Yale where she published a seminal dissertation, *Holyoke, Massachusetts: A Case History of the Industrial Revolution in America* (1939), her initial foray into urban history. She was teaching at Smith College when the coming of World War II led her to accept a post as historian of the Army Arsenal at Springfield, Massachusetts. Later she moved to Washington where she collaborated in writing the official history of the U.S. Army Ordnance Department.

Connie Green's gracious eighteenth-century home directly behind the Supreme Court building in Washington soon became a gathering place for visiting historians. As she put it, the old house had "classical frontage and Bohemian rearage." But

whatever the prospect, one was sure to find there both generous hospitality and, even more rewarding, a lively salon where Connie herself presided over the exchange of ideas with wry good humor and trenchant comment.

In 1964 Connie Green matched her father's performance when she too won a Pulitzer Prize with the first of her two-volume social history of the District of Columbia, *Washington, Village and Capital, 1800 to 1878* (1962-64). From this triumph she went on to break new ground with *The Secret City: A History of Race Relations in the Nation's Capital* (1967). Her success in tackling urban history did not, however, lead her to abandon her long-standing interest in the history of technology. When nearly 70 she responded to a request from the National Aeronautics and Space Agency and plunged into the intricacies of the space age to write, with Milton Lomask, *Vanguard, A History* (1971), an account of the nation's first satellite. For the advocates of second careers for women after raising families, Connie Green's performance will stand as vindication and inspiration.

I. B. HOLLEY, JR.  
Duke University

The death of WILLIAM GREENLEAF on December 17, 1975, at the age of 58 saddened the lives of colleagues, students, and scholarly associates alike. Born in Brooklyn, New York, he was educated in the New York public school system and the City College where he early demonstrated his exceptional proficiency as a writer. His master's essay and Ph.D. dissertation, both written at Columbia University, revealed an extraordinary combination of talents in research, analysis, and writing. The dissertation, a study of the Selden Patent Suit, litigation so central to the automotive industry, was a model in its comprehension of both the technology and the intricate legal issues involved. Subsequently published under the title *Monopoly on Wheels* (1961), it is regarded as one of the outstanding dissertations of the Columbia History Department. A research associate of the Ford Motor Company History Project directed by Allan Nevins, he collaborated on the multivolume biography of Henry Ford. He joined other historians in preparing several editions of the *Encyclopedia of American History* (1953-76), a documentary history, *The Rise of Industrial America, 1840-1900*, and a survey text, *U.S.A.: The History of a Nation*. At the time of his death, William Greenleaf was busily engaged in two disparate projects, testimony to his versatility as writer and historian—a history of the Civil War in the North for the *New American Nation Series* and a study of the early history of the motion picture industry in America.

Greenleaf's integrity as a scholar was matched by his generosity of time and assistance to others and the warm and self-effacing quality which marked his relations with students and colleagues both at Colorado State University, where he served briefly, and at the University of New Hampshire where he taught since 1958. There, in addition to earning a reputation as an outstanding classroom interpreter of recent and contemporary United States history, he initiated seminars and colloquia on various aspects of the economic history of the New Deal while carrying concurrently an active graduate program. In recent years he served as New Hampshire's representative on the University Press of New England. He is survived by three sons, Peter, Eric, and Allan, to whom his devotion is legendary.

RICHARD B. MORRIS  
Columbia University

S. HARRISON THOMSON, editor, scholar of international renown, dedicated teacher, and leader in the historical profession, died at the age of 80 in Boulder, Colorado, on November 19, 1975. Born in California, he pursued his studies in modern languages, literature, philosophy, theology, and history at Princeton, Oxford, and Charles University in Prague, Czechoslovakia. Prior to his professorship in medieval history at the University of Colorado, Boulder, where he taught from 1936 until 1964, Thomson had teaching positions at Princeton, California Institute of Technology, and the University of Chicago. He was also visiting professor at Indiana University (1950-51) and the University of Washington (1964-65) and held the Duke Chair of History at UCLA (1965-66).

Harrison Thomson's contributions to Latin paleography and to the history of the late middle ages and the Western Slavs include numerous articles, reviews, and books. Of particular significance are his independent commentaries and text editions *Magistri Johannis Hus Tractatus Responsivus*, *Magistri Johannis Wyclif Summa de Ente Tractatus Primus et Secundus*, *The Writings of Robert Grosseteste*, and *Magistri Johannis Hus Tractatus de Ecclesia*. His pioneer study *Czechoslovakia in European History* (1943) won him the State Prize of the Czechoslovak Republic in 1944, while *Europe in Renaissance and Reformation* (1963) appeared also in a German edition (1969). For the *Latin Bookhands of the Later Middle Ages* (1969), he was awarded the Haskins Medal of the Mediaeval Academy of America, and the American Association for the Advancement of Slavic Studies honored him with its Award for Distinguished Contributions to Slavic Studies in 1973.

In 1936, Thomson assumed the editorship of the *Progress of Medieval Studies in the United States and*



Canada. Five years later, he founded the *Journal of Central European Affairs*, the first English-language periodical devoted primarily to the specific problems of the area between the Russian and German lands. In 1943, he started *Medievalia et Humanistica*, editing, with the help of his wife, three periodicals for nearly a quarter-century. He also served on the editorial board of the *Slavic Review*.

As editor of important scholarly publications and with his meticulously researched writings set forth in an urbane yet forceful style, Thomson was a source of inspiration to both the established member of the profession and the novice. In his system of values scholarship ranked high, even though he was humbly aware of its limitations *sub specie aeternitatis*. Yet he took pride in his students' achievements, educating them with tact and example to become worthy of the name of historian. A master of classic and medieval Latin and half-dozen Indo-European languages, he felt challenged to learn Magyar in his late sixties.

Harrison Thomson left the privacy of his study in Boulder for frequent visits to archives and libraries all over the Western world. A leading expert on the religious and social turbulence of heretic movements, he could spend countless hours deciphering the handwriting of a scribe to determine the cultural milieu of a monastery. An admirable sense of proportion and catholicity of view in matters of learning made Thomson a respected cultural representative of the United States in Prague and Warsaw in 1945 and 1946 even while his international reputation as a research scholar continued to grow. He was a member of the Institute for Advanced Study at Princeton, a fellow and later first vice-president of the Mediaeval Academy of America, served as chairman and vice-chairman of the Western Slavic Conference, and actively participated in the affairs of the American Historical Association, the History of Science Society, the American Society for Church History, the Renaissance Society of America, the American Council of Learned Societies, and the Social Science Research Council. He was also an associate of the Société Philosophique de Louvain, corresponding fellow of the Polish Academy of Sciences in Cracow, honorary fellow of the Czech Academy of Arts and Sciences, a member of the Society for the Study of Medieval Languages and Literatures and the Anglo-Norman Text Society, both in Oxford, and a corresponding fellow of the Polish Academy of Arts and Letters in London.

To those former students and colleagues who knew him, Harrison Thomson was a reliable and humane friend. But the spiritual legacy he left goes beyond the boundaries of personal friendship. Indeed it represents American scholarship at its best, because it reflects the search for truth of a probing

mind and the humanitarian values of a great historian.

GEORGE BARANY  
University of Denver

SIR JOHN W. WHEELER-BENNETT, GCVO, CMG, OBE, FBA, died on December 9, 1975, in St. Thomas Hospital, London at the age of 72. His passing brings to an end a life that not only spanned, but also was often intimately connected with the tumultuous events of the first three-quarters of the twentieth century. He seemed always to be in the right place at the right time. He was in China in the early 1930s; he left Berlin for Prague the day of the "night of the long knives," having just dined with some of its prominent victims; and he interviewed Kaiser Wilhelm II and Leon Trotsky shortly before their deaths. Accounts of some of these encounters appeared in an anthology of Wheeler-Bennett's historical studies, *A Wreath to Clio* (1967). During his long and active career he was attached to the British Library of Information, British Press Service, British Political Warfare Mission in the United States, and Allied Military Government in Europe. A close associate of Winston Churchill, Anthony Eden, and Harold Macmillan, Sir John edited *Action This Day: Working with Churchill* (1968), a collection of excerpts from pertinent memoirs. In 1945 he married a gracious Virginia woman, Ruth Risher.

Wheeler-Bennett was an Honorary D.C.L. of Oxford University, an Honorary Fellow of St. Anthony's College, Oxford, and an Honorary D.Litt. of New York and Birmingham Universities. He was knighted in 1959 for his *King George VI: His Life and Reign* (1958) and served as Historical Advisor to the Royal Archives until his death. He taught at Oxford University, New York University, the Universities of Virginia and Arizona, and other important institutions in Europe and the United States.

Although Sir John considered himself a German historian, he was a recognized expert on international relations as well. His first major book was *Wooden Titan: Hindenburg in Twenty Years of German History, 1914-1934* (1936), but he himself and many others judged the next, *The Forgotten Peace, Brest-Litovsk, March, 1918* (1939), to be his finest effort. Other important works included *Munich: Prologue to Tragedy* (1948), *Nemesis of Power: The German Army in Politics, 1918-1945* (1954), *John Anderson, Viscount Waverly* (1962), and *The Semblance of Peace: The Political Settlement After the Second World War* (1972). Sir John also published numerous articles, some of them dealing with the American Civil War, which was a topic of abiding interest to him, in *Foreign Affairs*, *Virginia Quarterly Review*, *History Today*, and other journals.

Those of us who studied under Sir John and

knew him, respected him as a scholar, venerated him as a teacher, and cherished him as a friend. He was exemplar, counselor, and “living history” to us. Wherever Sir John and Lady Ruth happened to be living there was a warm welcome with good company and continuing inspiration for us.

He was deeply loved, and the absence of his commentary, wit, and consideration will leave the profession and us all the poorer.

DENNIS REINHARTZ  
*University of Texas,  
Arlington*

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## Other Books Received

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Books listed were received by the *AHR* between January 1 and March 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- BAKER, DEREK, editor. *The Materials, Sources and Methods of Ecclesiastical History: Papers Read at the Twelfth Summer Meeting and the Thirteenth Winter Meeting of the Ecclesiastical History Society.* (Studies in Church History, volume 11.) New York: Barnes and Noble. 1975. Pp. xii, 370. \$20.00.
- BANKS, ARTHUR. *A Military Atlas of the First World War.* With commentary by ALAN PALMER. New York: Tapplinger Publishing Company. 1975. Pp. xii, 338. \$29.95.
- BIRNBERG, THOMAS B., and RESNICK, STEPHEN A. *Colonial Development: An Econometric Study.* (Economic Growth Center, Yale University.) New Haven: Yale University Press. 1975. Pp. xiii, 347. \$20.00.
- BLUMENSON, MARTIN and STOKESBURY, JAMES L. *Masters of the Art of Command.* Boston: Houghton Mifflin Company. 1975. Pp. xiv, 393. \$12.50.
- CROSLAND, MAURICE, editor. *The Emergence of Science in Western Europe.* New York: Science History Publications. 1976. Pp. 201. \$18.00.
- DAVIS, DAVID BRION. *The Problem of Slavery in the Age of Revolution, 1770-1823.* Ithaca: Cornell University Press. 1975. Pp. 376. \$5.95.
- DJUVARA, NEAGU M. *Civilisations et lois historiques: Essai d'étude comparée des civilisations.* Paris: Mouton. 1975. Pp. 448, 65fr.
- DOBB, MAURICE, et al. *The Transition from Feudalism to Capitalism.* With an introduction by RODNEY HILTON. (Foundations of History Library.) Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 195. \$12.50.
- DUFF, WILSON. *Images Stone B.C.: Thirty Centuries of Northwest Coast Indian Sculpture.* Photographs and drawings by HILARY STEWART. Seattle: University of Washington Press. 1975. Pp. 191. \$17.50.
- EHRENBERG, RALPH E., editor. *Pattern and Process: Research in Historical Geography.* (National Archives Conferences, volume 9.) Washington, D.C.: Howard University Press. 1975. Pp. xv, 360. \$15.00.
- EISENSTADT, S. N., and AZMON, Yael, editors. *Socialism and Tradition.* (The Van Leer Jerusalem Foundation Series.) Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 262. \$10.00.
- FEHL, NOAH EDWARD. *Personality and Pattern in History.* Hong Kong: Chung Chi Publications. 1975. Pp. xiv, 171. \$8.00.
- FOWLER, PETER J., editor. *Recent Work in Rural Archeology.* Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 160. \$13.00.
- GILLISPIE, CHARLES COULSTON, et al., editors. *Dictionary of Scientific Biography.* Volume XIII, *Hermann Staudinger-Giuseppe Veronese.* New York: Charles Scribner's Sons. 1976. Pp. xiii, 623. \$40.00.
- GOLDMAN, ISRAEL M. *Lifelong Learning Among Jews: Adult Education in Judaism from Biblical Times to the Twentieth Century.* With an introduction by LOUIS FINKELSTEIN. New York: Ktav Publishing House. 1975. Pp. xxii, 364. \$15.00.
- GRAETZ, HEINRICH, translator and editor. *The Structure of Jewish History and Other Essays.* (Studies in Jewish History, Literature, and Thought Series: Moreshet, volume 3.) New York: Jewish Theological Seminary of America. 1975. Pp. x, 325. Cloth \$15.00, paper \$4.95.
- LOWENTHAL, DAVID, and BOWDEN, MARTYN J., editors. *Geographies of the Mind: Essays in Historical Geosophy in Honor of John Kirtland Wright.* New York: Oxford University Press. 1976. Pp. 263. \$9.00.
- PISTONE, SERGIO, editor. *L'idea dell'unificazione europea dalla prima alla seconda guerra mondiale: Relazioni tenute al convegno di studi svoltosi presso la Fondazione Luigi Einaudi (Torino, 25-26 ottobre 1974).* (Studi, 21.) Turin: Fondazione Luigi Einaudi. 1975. Pp. 243. L.3,500.
- ROBERTS, JOHN. *Revolution and Improvement: The Western World, 1775-1847.* Berkeley and Los Angeles: University of California Press. 1976. Pp. xii, 290. \$20.00.
- SMITH, PAUL, editor. *The Historian and Film.* New York: Cambridge University Press. 1976. Pp. viii, 208. \$13.95.
- STEPHENS, LESTER D., compiler and editor. *Historiography: A Bibliography.* Metuchen, N.J.: Scarecrow Press. 1975. Pp. vi, 271. \$9.00.
- WAGNER, ANTHONY. *Pedigree and Progress: Essays in the Genealogical Interpretation of History.* London: Phillimore and Co. 1976. Pp. 333. \$25.00.
- WESTMAN, ROBERT S., editor. *The Copernican Achievement.* (UCLA Center for Medieval and Renaissance Studies Contributions, 7.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 405. \$14.50.
- WOODWARD, DAVID, editor. *Five Centuries of Map Printing.* Chicago: University of Chicago Press. 1975. Pp. xi, 177. \$17.50.

### ANCIENT

- BONFANTE, LARISSA. *Etruscan Dress.* Baltimore: Johns Hopkins University Press. 1976. Pp. ix, 243. \$17.50.
- Byzantine Books and Bookman.* (Dumbarton Oaks Colloquium, 1971.) Washington, D.C.: Dumbarton Oaks Center for Byzantine Studies. 1975. Pp. x, 109.
- CARY, M., and SCULLARD, H. H. *A History of Rome Down to the Reign of Constantine.* 3d ed. New York: St. Martin's Press. 1976. Pp. xxvii, 694. \$14.95.
- EHRENBERG, VICTOR. *The People of Aristophanes: A Sociology*

- of *Old Attic Comedy*. New York: Barnes and Noble, 1974. Pp. xii, 385. \$19.00.
- FOSS, CLIVE. *Byzantine and Turkish Sardis*. (Archaeological Exploration of Sardis, 4.) Cambridge, Mass.: Harvard University Press, 1976. Pp. xvi, 216. \$12.50.
- GRINSELL, LESLIE V. *Barrow, Pyramid and Tomb: Ancient Burial Customs in Egypt, the Mediterranean and the British Isles*. (The World of Archaeology.) Boulder, Colorado: Westview Press, 1975. Pp. 240. \$18.50.
- HADINGHAM, EVAN. *Circles and Standing Stones: An Illustrated Exploration of Megalith Mysteries of Early Britain*. New York: Walker and Company, 1975. Pp. vii, 240. \$12.50.
- HAMMOND, N. G. L. *The Classical Age of Greece*. (History of Civilizations.) New York: Barnes and Noble, 1976. Pp. xi, 308. \$27.50.
- ROBERTSON, MARTIN. *A History of Greek Art*, in two volumes. New York: Cambridge University Press, 1975. Pp. xviii, 611; v, 613-835.

## MEDIEVAL

- BARRACLOUGH, GEOFFREY. *The Crucible of Europe: The Ninth and Tenth Centuries in European History*. Berkeley and Los Angeles: University of California Press, 1976. Pp. 180. \$14.95.
- FOLDA, JAROSLAV. *Crusader Manuscript Illumination at Saint-Jean d'Acree, 1275-1291*. Princeton: Princeton University Press, 1976. Pp. xxix, 231. \$35.00.
- LEWIS, ARCHIBALD. *Knights and Samurai: Feudalism in Northern France and Japan*. London: Temple Smith, 1975. Pp. 101. \$7.75.
- Medievalia et Humanistica: Studies in Medieval and Renaissance Culture*. (New Series, number 6, *Medieval Hagiography and Romance*, edited by PAUL MAURICE CLOGAN.) New York: Cambridge University Press, 1976. Pp. xiii, 223. \$19.95.
- RAFTIS, J. A. *Assart Data and Land Values: Two Studies of the East Midlands, 1200-1350*. (Subsidia Mediaevalia, 3.) Toronto: Pontifical Institute of Mediaeval Studies, 1974. Pp. 169. \$8.50.
- STORM, GUSTAV, editor. *Latinske Kildeskrifter til Norges Historie I Middelalderen*. (Momumenta Historica Norvegiae, Latine Conscripta.) Reprint. Oslo: Norsk Historisk Kjeldeskrift-Institut. 1973. Pp. lxii, 301.
- WALEY, DANIEL. *Later Medieval Europe from Saint Louis to Luther*. Rev. ed. New York: Longman, 1975. Pp. xiv, 306. \$7.50.
- YATES, FRANCES A. *The Valois Tapestries*. New ed. Boston: Routledge and Kegan Paul, 1976. Pp. xxvii, 150, xii. \$26.25.
- GLASSCOCK, ROBIN E., editor. *The Lay Subsidy of 1334*. (Records of Social and Economic History, New Series, 2.) New York: Oxford University Press, for The British Academy, 1975. Pp. xxxvii, 516. \$36.50.
- GODFREY, ELEANOR S. *The Development of English Glassmaking, 1560-1640*. Chapel Hill: University of North Carolina Press, 1975. Pp. xii, 288.
- GRAVES, EDGAR B., editor. *A Bibliography of English History to 1485*. New York: Oxford University Press, 1975. Pp. xxiv, 1103. \$52.00.
- HALL, A. RUPERT, and HALL, MARIE BOAS, editors and translators. *The Correspondence of Henry Oldenburg*. Volume 10, *June 1673-April 1674, Letters 2241-2489*. London: Mansell; distrib. by International Scholarly Book Services, Beaverton, Ore. 1975. Pp. xxvii, 596. \$35.00.
- HALL, A. RUPERT, and TILLING, LAURA, editors. *The Correspondence of Isaac Newton*. Volume 5, *1709-1713*. New York: Cambridge University Press, for the Royal Society of London, 1975. Pp. li, 439. \$55.00.
- HANHAM, ALISON, editor. *The Cely Letters, 1472-1488*. (Early English Text Society, number 273.) New York: Oxford University Press, 1975. Pp. xxvii, 365. \$15.00.
- HILL, B. W., editor. *Edmund Burke on Government, Politics and Society*. New York: International Publications Service, 1976. Pp. 382. \$15.00.
- HILTS, VICTOR L. *A Guide to Francis Galton's English Men of Science*. (Transactions of the American Philosophical Society, New Series, volume 65, part 5.) Philadelphia: The American Philosophical Society, 1975. Pp. 85.
- KENNEDY, KIERAN A., and DOWLING, BRENDAN R. *Economic Growth in Ireland: The Experience Since 1947*. New York: Barnes and Noble, in association with The Economic and Social Research Institute, Dublin, 1976. Pp. xix, 345. \$22.50.
- KILLIP, MARGARET. *The Folklore of the Isle of Man*. Drawings by NORMAN SAYLE. (The Folklore of the British Isles.) Totowa, N.J.: Rowan and Littlefield, 1976. Pp. 207. \$15.00.
- LERUEZ, JACQUES. *Economic Planning & Politics in Britain*. Translated by MARTIN HARRISON; preface by J. E. S. HAYWARD. New York: Barnes and Noble, 1976. Pp. xi, 324. \$29.00.
- MCLEAN, IAIN. *Keir Hardie*. (British Political Biography.) New York: St. Martin's Press, 1975. Pp. viii, 183. \$12.95.
- MCNEILL, PETER, and NICHOLSON, RANALD, editors. *An Historical Atlas of Scotland c.400-c.1600*. Fife, Scotland: Atlas Committee of the Conference of Scottish Medievalists, 1975. Pp. x, 213. \$6.00.
- MINGAY, G. E., editor. *Arthur Young and His Times*. Toronto: Macmillan Company, 1975. Pp. 264. \$26.50.
- PARSONS, DAVID, editor. *Tenth-Century Studies: Essays in Commemoration of the Millennium of the Council of Winchester and Regularis Concordia*. London: Phillimore and Co. 1976. Pp. xiii, 270. \$25.00.
- PEELE, GILLIAN, and COOK, CHRIS, editors. *The Politics of Reappraisal, 1918-1939*. New York: St. Martin's Press, 1975. Pp. viii, 265. \$15.95.
- REID, R. R. *The King's Council in the North*. Reprint. Totowa, N.J.: Rowman and Littlefield, 1976. Pp. x, 532. \$30.00.
- ROBERTS, PHILIP, editor. *The Diary of Sir David Hamilton, 1709-1714*. New York: Oxford University Press, 1975. Pp. xlviii, 138. \$28.00.
- THOMAS, PETER D. G. *Lord North*. (British Political Biography.) New York: St. Martin's Press, 1975. Pp. viii, 176. \$12.95.

## BRITISH COMMONWEALTH AND IRELAND

- BATTS, JOHN STUART. *British Manuscript Diaries of the Nineteenth Century: An Annotated Listing*. Totowa, N.J.: Rowman and Littlefield, 1976. Pp. xi, 345. \$25.00.
- BINGHAM, CAROLINE. *The Kings & Queens of Scotland*. New York: Taplinger Publishing Company, 1976. Pp. xi, 182. \$9.95.
- CLARKSON, LESLIE. *Death, Disease and Famine in Pre-industrial England*. New York: St. Martin's Press, 1975. Pp. 188. \$17.95.
- COOK, CHRIS, compiler. *Sources in British Political History, 1900-1951*. Volume 2, *A Guide to the Private Papers of Selected Public Servants*. With PHILIP JONES et al. New York: St. Martin's Press, 1975. Pp. 297. \$16.95.
- DARBY, H. C., and VERSEY, G. R. *Domesday Gazetteer*. New York: Cambridge University Press, 1975. Pp. viii, 544. \$72.50.
- DERRY, JOHN W. *Castlereagh*. (British Political Biography.) New York: St. Martin's Press, 1976. Pp. viii, 247. \$15.95.

## FRANCE

- CHURCH, WILLIAM F. *Louis XIV in Historical Thought: From Voltaire to the Annales School*. (A Norton Series: Histori-

- cal Controversies.) New York: W. W. Norton and Company. 1976. Pp. 127. Cloth \$6.95, paper \$1.95.
- COMTE, AUGUSTE. *Correspondance générale et confessions*. Volume 2, avril 1841-mars 1845. Compiled by PAULO E. DE BERRÊDO CARNEIRO and PIERRE ARNAUD. (Archives Positivistes.) Paris: Mouton. 1975. Pp. xxxvi, 461. 98 fr.
- SZAJKOWSKI, ZOSA. *Jews and the French Foreign Legion*. New York: Ktav Publishing House. 1975. Pp. 280. \$17.50.

## SPAIN AND PORTUGAL

- ALMUIÑA FERNÁNDEZ, CELSO. *Teatro y cultura en el Valladolid de la ilustración: los medios de difusión en la segunda mitad del xviii*. With a prologue by L. M. ENCISO RECIO. (Colección de Publicaciones Municipales, number 3.) Valladolid, Spain: Ayuntamiento de Valladolid. 1974. Pp. xxxii, 245. 250 ptas.
- GÓMEZ-IBÁÑEZ, DANIEL ALEXANDER. *The Western Pyrenees: Differential Evolution of the French and Spanish Borderland*. (Oxford Research Studies in Geography.) New York: Oxford University Press. 1975. Pp. xiv, 162. \$16.75.
- GONZÁLEZ JIMÉNEZ, MANUEL. *La repoblación de la zona de Sevilla durante el siglo XIV: Estudio y documentación*. (Anales de la Universidad Hispalense, Serie, Filosofía y Letras, number 28.) Seville: Publicaciones de la Universidad de Sevilla. 1975. Pp. 164.
- HANSON, CARL A. *Dissertations on Iberian and Latin American History*. New York: Whitson Publishing Company. 1975. Pp. v, 400.
- RODRÍGUEZ GARRAZA, RODRIGO. *Tensiones de Navarra con la Administración Central (1778-1808)*. Pamplona: Diputación Foral de Navarra, Institución Príncipe de Viana, Consejo Superior de Investigaciones Científicas. 1974. Pp. 360.
- SÁNCHEZ-ALBORNOZ, NICOLÁS. *Los precios agrícolas durante la segunda mitad del siglo XIX*. Volume 1, *Trigo y cebada*. (Materiales para la Historia Económica de España.) Madrid: Servicio de Estudios del Banco de España. 1975. Pp. xii, 283.

## LOW COUNTRIES

- The Correspondence of Erasmus: Letters 142 to 297, 1501 to 1514*. Translated by R. A. B. MYNORS and D. F. S. THOMSON. Annotated by WALLACE K. FERGUSON. (Collected Works of Erasmus, volume 2.) Buffalo: University of Toronto Press. 1975. Pp. xiii, 374. \$25.00.
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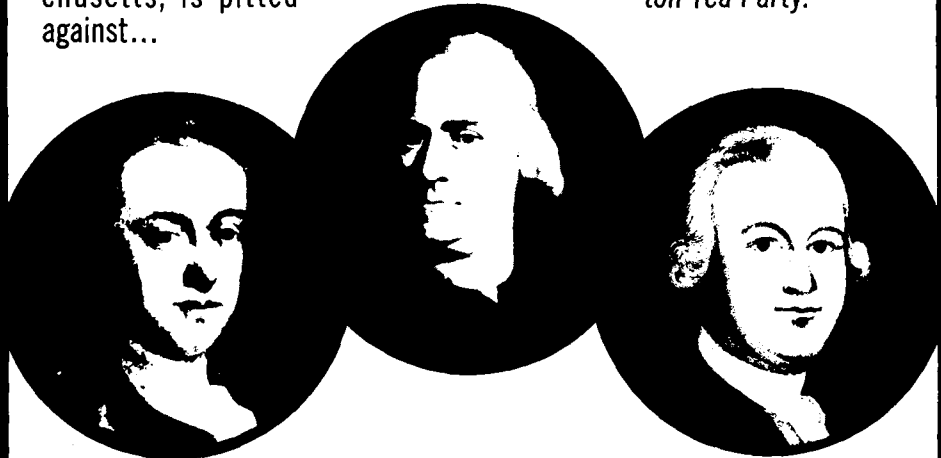
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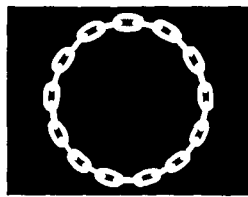
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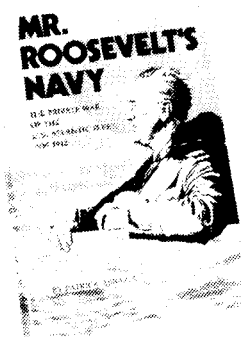
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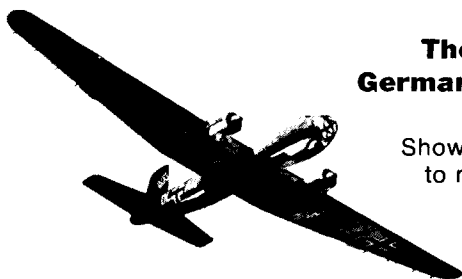
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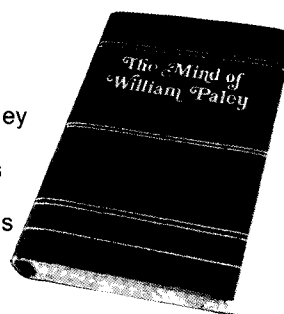
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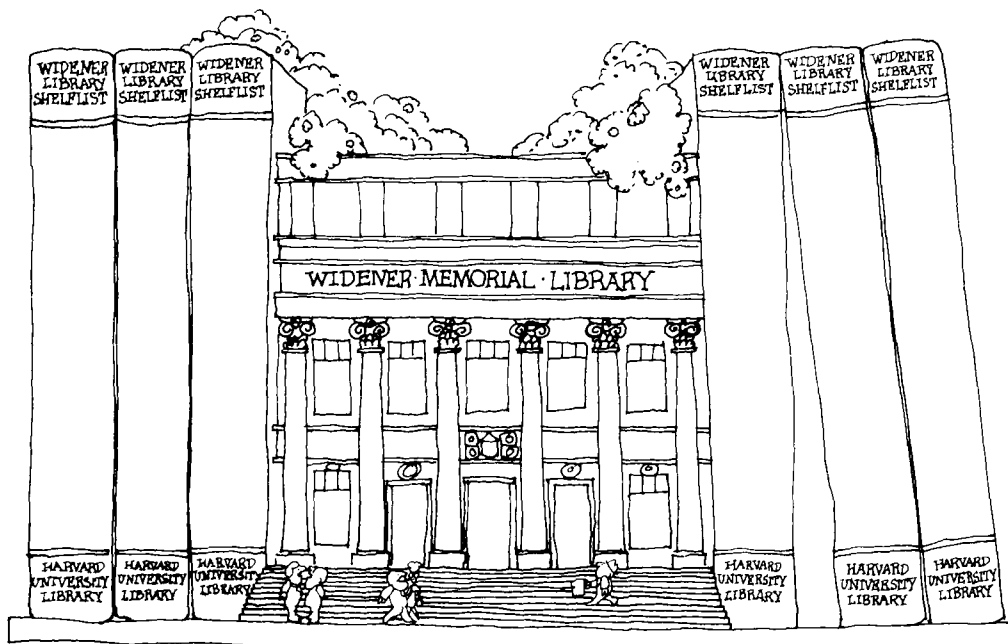
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